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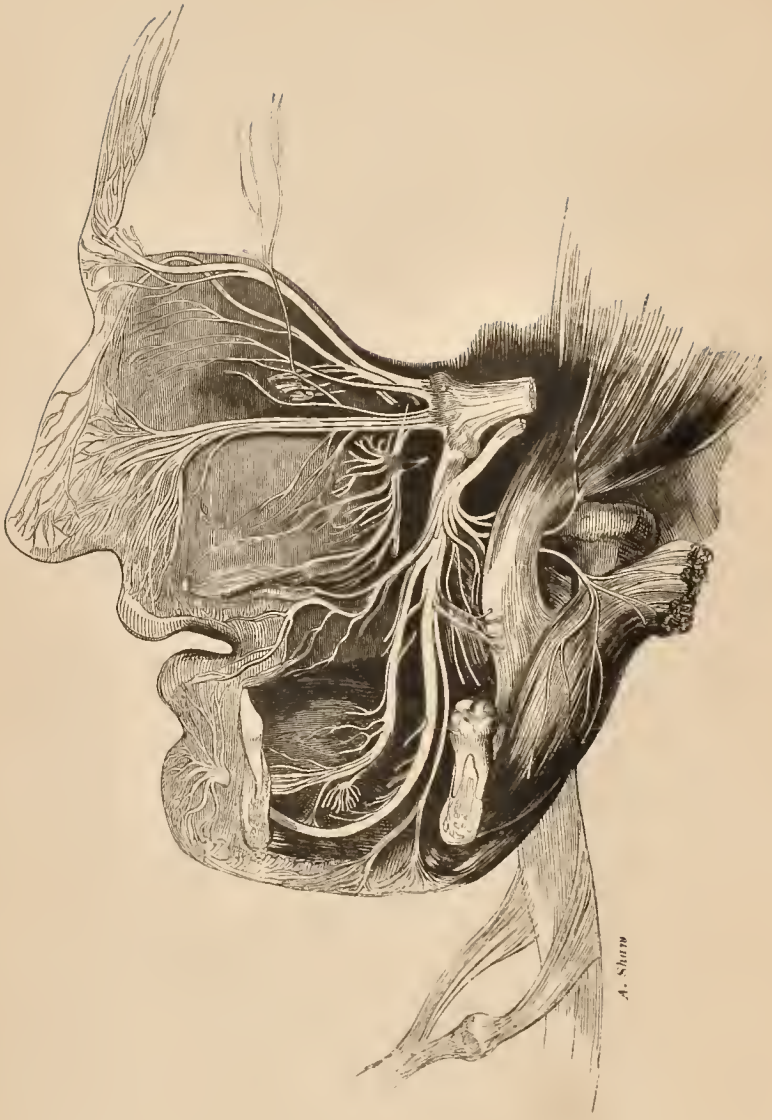

FIFTH PAIR, OR SPINAL NERVE OF THE HEAD.

NARRATIVE

OF THE

DISCOVERIES OF SIR CHARLES BELL

IN THE

NERVOUS SYSTEM.

BY

ALEXANDER SHAW,

ASSISTANT-SURGEON TO THE MIDDLESEX HOSPITAL.

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INTRODUCTION.

PREVALENT MISTATEMENTS CONCERNING THE ORIGIN OF THE DISCOVERIES IN THE NERVOUS SYSTEM DESCRIBED.

The following pages are intended to give a history of the recent discoveries in the Nervous System. They present a view of the general principle on which the late improvements in this department of physiology have been founded; and also shew in how far we are indebted to different physiologists for propounding and confirming that principle.

That such a history should be required at the present time, will perhaps appear remarkable. But I am impressed with the conviction, that, whoever takes the trouble to peruse these pages, will admit, that the task I am about to enter upon was not uncalled for; and that it devolved upon me to undertake it.

The immediate cause of my entering on the subject has been, that, in different publications of general and extensive circulation, statements altogether unfounded, and obviously proceeding from one common source, have recently been appearing with remarkable frequency, concerning the views originally expressed by

Sir Charles Bell, as to the functions of certain important parts of the nervous system.

The statement to which I particularly refer, is to this effect :—that when Sir Charles Bell first published on the Nerves, he entertained the opinion, that each root of the spinal nerves possessed two distinct endowments:—that is, instead of his supposing that the power of regulating the muscles belonged to one of the two roots of these nerves, and the power of conveying sensation to the other, it is alleged, that he supposed that both motion and sensation resided conjointly in the anterior root, and the power of controlling the growth and the sympathies of the parts in the posterior. These opinions, it is further said, he continued to hold, until it was discovered by certain other gentlemen who engaged in the same inquiries, that the anterior root was subservient to motion, and the posterior to sensation.*

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* "But Sir Charles Bell appears, at first, to have pursued his researches with reference, not to this distinction, but to others. In his 'Idea of a New Anatomy of the Brain' (1811), he endeavoured to establish the opinion, that different endowments are in the same cord of the spinal nerves, and held by the same sheath; and the nature of this difference was, that both sensation and volition belonged to the anterior origin of the nerve, and another function to the posterior origin."—The Rev. Mr. Whewell, "*London Medical Gazette*," vol. xxi. p. 526.

"Every spinal nerve rises by two roots; one from the anterior, and the other from the posterior, column of the spinal cord. Sir Charles Bell supposed both sensation and motion to depend upon the anterior root; the posterior root controlling the growth and sympathies of parts. Magendie and Mayo believe the anterior root to govern motion, and the posterior root

Now if the above account were well founded, the following conclusion would necessarily be drawn:—namely, that on first promulgating his views, Sir Charles Bell had been ignorant, not only of the proper functions of the important series of nerves in question (which supply nearly the whole body), but of the fundamental principle which has been the original source of all the discoveries; and that he must, therefore, have been universally wrong in all his reasonings and deductions concerning the distinctions of the nervous system.

To illustrate what is here advanced, and at the same time to allow the reader to understand correctly what really constitutes the discoveries in question, let me explain the nature of this important principle.

Formerly it was conceived, that to all the nerves of the body, indiscriminately, there belonged an indefinite number of functions—vaguely termed nervous.

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to influence sensation."—*The "Times" Newspaper, in the notice of a work by* M. Joubert.

"The double-rooted origin of the nerves has some striking circumstances connected with it, which were first partially pointed out by Sir Charles Bell, and afterwards more fully elucidated by Magendie. When a spinal nerve is divided any where after the junction of its two roots, the parts of the body to which it is distributed lose both sensation and the power of motion. 'But if the two origins,' says Mr. Mayo, 'of a spinal nerve are exposed in a young animal, and separately divided, different effects are produced. The section of the anterior root deprives the part supplied by the nerve of voluntary motion, sense remaining; the section of the posterior root deprives the corresponding part of the body of sensation, voluntary motion being left.' By some inexplicable law, therefore," &c.—*Chambers' Edinburgh Journal*, No. 327.

The power of controlling the muscles was one of these; the power of conveying sensation from the integuments was another; and there might be additional ones, to which it is not easy to apply any precise name. The opinion, however, prevailed universally —that each nerve proceeding from the brain or spinal marrow possessed several endowments, but at least the two—motion and sensation.

This view of the nerves was connected with a similarly erroneous idea respecting the nature of the brain and spinal marrow. These parts, it was conceived, possessed equal powers, throughout their whole substance, of propagating to the body the peculiar properties supposed to be transmitted by the nerves. For example, taking any division of the brain or spinal marrow, it was considered that this particular part was as capable of dispensing to the muscles the power of motion, or of receiving sensation from the integuments, as any other division. The brain and spinal marrow, in short, were regarded as forming a certain central reservoir of nervous power; and whatever nerve was connected with either of them could bestow, of course, both motion and sensation.

The reason, it may be conjectured, why physiologists laboured so long under these erroneous views, was this:—they placed too much confidence in the results of experiments on living animals, and neglected the anatomy. In all parts of the body (with the exception of the head), the trunks of the nerves are really compound. Take any nerve of the arm—the median or ulnar; or of the lower extremity—the ischiatic or ante-

rior crural, and submit it to experiment, and it will be discovered to possess both motion and sensation. When cut across, the animal will be insensible to the prick of the instrument below where the nerve has been divided, in consequence of the power of conveying sensation being destroyed; and the muscles will be paralysed, owing to the motor power having been lost. Such observations, it is natural to suppose, misled physiologists, and made them believe that sensation and motion were inherent properties, belonging to all nerves indiscriminately.

The new inquiries, however, in the nervous system have overturned these views. Instead of its being now supposed that the nerves are equally capable of bestowing motion or sensation, it has been discovered, that for each of these endowments distinct nerves are appropriated. Again, instead of its being imagined that the brain and spinal marrow consist of homogeneous parts, ministering to these two functions equally, it has been demonstrated, that one distinct division is provided for motion exclusively, and another for sensation exclusively.

Now, to the mode in which these facts have been established, I entreat the reader's undivided attention; because, upon what I am about to state, the whole question as to the originality of the discoveries immediately turns.

What, then, has been the method referred to, of demonstrating that the nerves of sensation are distinct from those of motion? It has been simply this:—by tracing the nerves to their connexions with the brain

and spinal marrow, and investigating the nature of their *roots*. In other words, it has been by considering whether the nerves, which, in their course through the body, possess both motion and sensation, may not consist of two distinct sets of filaments enveloped in the same sheath; and whether by following these filaments to their origin, it may not be found that those appropriated for motion form a distinct root, or proceed from a distinct tract or subdivision of the brain or spinal marrow, from those which bestow sensation. Accordingly, by taking this view it will be seen, that, to pursue the inquiry successfully by experiment, it is necessary, in place of selecting the nerves themselves, as before, to fix upon their roots for observation.

To illustrate the principle further. If it be conceived that, instead of each of the different subdivisions of the brain and spinal marrow being enabled, as was formerly supposed, to give out various kinds of nervous power, each particular division has an appropriate office to perform, we may comprehend how the nerves proceeding from these distinct parts may be endowed with corresponding appropriate functions. If, for example, on examining the brain or spinal marrow we observe, that from one prolonged tract a series of nerves arises in a continuous line, while from another tract a different series of nerves arises in a similar manner, it is natural to conjecture that the one column will minister to a certain function, while the other column will minister to a different, and that the roots or nerves connected with each, will possess corresponding endowments with the columns from which they arise. If such an hypo-

thesis be allowed, it will follow, that when the nerves in question, after escaping from the vertebral canal or skull, unite with each other, so that their fibrils are enveloped in the same sheath, they will become compound nerves, and combine the two powers of bestowing motion and sensation.

Here, then, is the simple explanation of the principle on which all these new discoveries have been based. It consists, I repeat, in supposing that, to investigate the functions of the nervous system successfully, we must devote our attention, not to the trunks, as was formerly done, but to the roots of the nerves. Accordingly, whoever was the first to suggest and follow out that new method of prosecuting the subject, must be declared the true originator of the recent improvements in this department of physiology. It is by the test of who did the most to establish this law, that we must decide to whom we are indebted for these discoveries.

But it will be perceived, that if Sir Charles Bell, in treating of the spinal nerves, really did express the opinions relating to their roots which have been imputed to him, he can have no claim to be considered a discoverer. The views attributed to him are of such a nature, that, if he entertained them, it is obvious he could not have had any conception of the truth of the principle above described, far less have been the first to propound it. Accordingly, it is but justice towards this gentleman to inquire, Whether there be any foundation for the statement in question? or, If this representation of his views be not, on the contrary, directly opposed to what he really has stated?

That the latter is the case, I am prepared to shew. From the evidence I can bring forward, it will be acknowledged that Sir Charles Bell, instead of having expressed, in any of his writings, the erroneous views imputed to him, has not only never entertained them, but has invariably, from the beginning, held and given out the very opinions which are ascribed to the other physiologists referred to;—that, so far back as eleven years before any of the gentlemen, regarded as his competitors, had written a single memoir on the subject of the nerves, he had both explained the principle above described in the clearest manner, and employed it with success in his investigations.

Applying the principle, in the first instance, to the nerves of the spine, he ascertained that the anterior root was subservient to motion alone, while the posterior was provided for sensation alone; and that it was owing to these two roots coalescing, and mingling their fibrils together, that the trunks, in their course through the body, possessed the two properties conjointly. But, besides this—long before he could have known that any other individual contemplated joining in the same inquiries—he had pursued the subject to a much further extent. That is, he had applied the same mode of investigation, namely, by examining the differences in their roots, to the nerves of the brain; and had demonstrated, by the most conclusive evidence, that the principle in question held good equally with regard to them as with regard to the spinal nerves.

For example, he selected two of the cerebral nerves

in particular, namely, the portio dura and the fifth pair. The first of these arises from the brain by a single root; while the latter arises by a double root, in that respect resembling the spinal nerves. According to the principle it would follow, that if the functions of the nerves corresponded with their roots, the portio dura would only possess one endowment; while the fifth pair, like the spinal nerves, would have two. Now this was exactly the result that he obtained. By experiments performed upon the portio dura, he demonstrated that it was a motor nerve exclusively, and had no power of bestowing sensation. When cut across, in the living animal, the motions of a certain set of muscles were immediately arrested, but the sensibility of the surface supplied by the nerve remained undiminished. Upon submitting the fifth pair to experiment, a totally different set of phenomena presented themselves. This nerve, although it arises from the brain by two roots, has one of its origins nearly four times larger than the other;* consequently, one of the roots passes to parts of the head where the other does not accompany it: and thus certain of the branches of the fifth are simple in their structure, while a different set are componnd. It was found, by experiment, that when those branches which proceed simply from the larger root were cut across, only one endowment, namely, sensation, was destroyed; whereas, upon cutting across those branches in which the fibrils of the two roots were united together in the

* See the Frontispiece.

same sheath, not only sensation, but the power of motion, were destroyed. It was from obtaining these results, that Sir Charles Bell applied to the portio dura a name which indicated that it gave motor power exclusively; while to the fifth pair he applied the name, "Nerve of sensation and mastication;" to shew that, although it conveyed sensation from all parts of the head, it bestowed motion, by a limited number of its branches alone, to the muscles employed in mastication.

Accordingly, it may be perceived how admirably the observations on these two cerebral nerves confirmed the principle, that the functions of the nerves bear a relation to the roots by which they arise from the brain. It is also apparent how directly they corroborated the conclusions drawn from the experiments on the roots of the spinal nerves. It deserves, therefore, the reader's especial notice, that when the description of Sir Charles Bell's original experiments on the nerves of the spine was given to the public, it was accompanied with an account of the experiments on the two nerves of the brain, which I have related: and these were brought forward, as will presently be seen, for the express object of verifying the statements made concerning the functions of the roots of the spinal nerves. Hence, before any other individual had composed a single paper on the functions of the nervous system, or before it could be conjectured that any one else intended to make these inquiries the subject of his study, not only had a correct account of the functions of the roots of the spinal nerves been given,

but it was accompanied with an exposition of the distinct uses of the portio dura and fifth pair, for the avowed object of corroborating the views therein announced.

And this was not all. Before a competitor had appeared, or before any one else professed to be acquainted even with the natural, healthy functions of the roots of the spinal nerves, certain important observations on the pathology of these nerves had been communicated to the public. As in the paper to which I am about to refer, it had been previously shewn, by a numerous series of cases, amounting to not less than twelve altogether, that when the portio dura was injured by any disease, involving its trunk, the motions of the features were arrested, while sensation remained perfect; and that when the superficial branches of the fifth pair were similarly affected by disease, sensation was destroyed, while the motions of the features were retained: thus shewing, that paralytic affections, formerly considered as the precursors of apoplexy, might depend upon disease situated external to the skull, and, therefore, might be unconnected with lesion of the brain. So an attempt was made to apply the new observations on the roots of the spinal nerves, in a similar manner, to the pathology of the spinal marrow. Certain affections of the upper or lower extremities, supplied by these spinal nerves, sometimes occur, in which the sensation of the limb is destroyed, while the motion remains entire; and *vice versâ*. Now, as it had been established experimentally, that motor power belonged to the anterior roots, and sensation to the

posterior, it was concluded that when motion, in these cases, was lost, it depended on a morbid condition of the anterior or motor column of the spinal marrow; while, if sensation was lost, it depended on disease of the posterior or sensitive column.

It is now incumbent on me to bring forward some part of the evidence on which I have ventured to make these statements. For this purpose, I prefer making use of a paper published by my late brother, Mr. John Shaw, who, at the time of composing it, had been, for fourteen years, Sir Charles Bell's pupil and assistant. This paper was entitled "On Partial Paralysis," and was communicated to the Medico-Chirurgical Society of London. It was read before that Society in April, 1822; and published in their "Transactions" in June of the same year. As it was not till the following August, that is, five months after the reading of Mr. Shaw's paper, that M. Magendie — to whom, with Mr. Mayo, the honour of originating the discoveries has been ascribed—published his memoir on the spinal nerves; * and it was not till July of the succeeding year, that is, a year and four months after the time referred to, that Mr. Mayo wrote on the same subject, † it is obvious, that Mr. Shaw could not have profited by the observations of either of these gentlemen.

"I shall take the liberty," Mr. John Shaw remarks, "of trespassing still more upon the time of the Society, by making

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* Journal de Physiologie Expériment. Tom. I. Août, 1822.

† Anatomical and Physiological Commentaries. No. II. July, 1823.

a few remarks upon a very curious question, which has particularly excited the attention of physicians in all ages, since the time of Galen.

" *Why Sensation should remain entire in a limb, when all Voluntary Power over the action of its Muscles is lost; or why Muscular Power should remain, when Feeling is gone?*

" The attention of Galen was particularly directed to this question, in consequence of his having been called upon by some of his contemporaries to account for the manner in which he had cured a partial paralysis of the finger, by applications made to the spine. In answer, Galen told them, that two sets of nerves went to every part: one, to endow the skin with sensibility; the other, to give the muscles the power of voluntary action. This opinion was probably founded on a mere theory: but the facts lately discovered, and the observations which have been noted in attending to the phenomena of disease, though they do not afford absolute proofs of the correctness of Galen's supposition, still go far to establish the fact, — *that every part of the body which is endowed with two or more powers, is provided with a distinct nerve for each function.*

" The form of the nerves, which at the same time endow the skin with sensibility and the muscles with the power of voluntary motion, is such, that they appear to be single cords; but if we examine the origin of any of these nerves, we shall find that it is composed of two packets of fibres, which arise from distinct parts of the spinal marrow. These origins are soon enveloped in the same sheath, so as to appear to a superficial observer to form a single nerve.

" It is not too much to suppose, that either of these origins may be affected while the other remains entire. To prove this by ocular demonstration will perhaps be impossible; and, therefore, the question will probably remain undecided. *But*

we have already seen examples of the consequences of injury to a nerve that has a single root, viz. the portio dura: for if we cut it, there will be only one set of actions paralysed; while by dividing a nerve which has a double origin, viz. the fifth, we shall destroy two powers, namely, voluntary motion and sensibility. We know also, that when we cut through the trunk of a nerve going to the hand, we destroy both sensibility and the power of motion.

"In reference to this subject, I shall state the result of certain experiments which were made about thirteen years ago, by Mr. Charles Bell. The two sets of filaments by which each spinal nerve is connected to the spinal marrow were exposed. On irritating one set, convulsions of the muscles on which the nerve was distributed, ensued; but when the other was excited, no perceptible effect was produced. These experiments we have often repeated, and always with the same results. But from the violence necessarily used in making them, it has been difficult to ascertain which of the filaments bestows sensibility on the part. It was easily shewn, that if only the posterior set was destroyed, the voluntary power over the muscles continued unimpaired; but the pain necessarily attendant upon the performance of the experiment, prevented us from judging of the degree of sensibility remaining in the part.

"It was, I believe, the result of these experiments which induced Mr. Bell to give an opinion nearly similar to that of Galen, in a short 'Essay on the Anatomy of the Brain,' printed and distributed among his friends in 1809.*

"If the view which I have here taken of this question be correct, it may lead to this rule of practice. If only one set of functions of a spinal nerve be deficient, we should apply our

* Correctly, 1811.—A. S.

remedies to that part of the system from which the nerve arises; but if both functions are impaired, we must then direct our inquiries to the state of the nerve in the whole course, from its origin to its distribution, as the loss of power is probably owing to some affection of a part of the nerve, after the two sets of filaments by which it arises are united together."

Where is there, in any part of the above extract, an expression to warrant the statements just noticed respecting Sir Charles Bell's original views? Where are the words, "growth and sympathies of the parts," the two qualities said to have been assigned by him to the posterior roots, to be found? Where is there any indication of its having been supposed that both motion and sensation resided in the same roots,—namely, the anterior roots? Does not every line contradict such a notion? At what time, or in what work, did Sir Charles Bell express these alleged opinions? From the extract it is perceived, that, so far back as 1811, this gentleman had composed an Essay on the Brain, in which he treated of the functions of the roots of the nerves in question; and it is particularly stated, that the views contained in that work coincided with those delivered by Mr. Shaw: Was there any earlier essay than that, wherein the opinions referred to might have been espoused? I say, that no such earlier production was ever published. But, even if evidence of that kind could be adduced, of what consequence would it be? I repeat, that the paper on "Partial Paralysis," from which the above quotation has been taken, was published five months before M. Magendie

undertook his experiments; and sixteen months before Mr. Mayo commenced his inquiries; and the views contained in it, I need not say, are the very same which it is pretended these gentlemen were the first to communicate to the public.

It devolves on me, therefore, to explain how statements so unfounded could have originated. For this object, I am compelled to direct the reader's attention to the work of one of the gentlemen who puts himself forward as a competitor for the honour of the discoveries. The following extracts from the "Outlines of Physiology," by Mr. Mayo, a work enjoying an extensive circulation, will leave no doubt that it has been by adopting, without any question, the account of the researches given by this gentleman, that the misconceptions above noticed have arisen.

"When a spinal nerve," Mr. Mayo observes,* "is divided, in its course through the body, the parts supplied by it, beyond the division, are paralysed—they lose sense and motion. If the two origins of the spinal nerves are exposed in a young animal, and separately divided, different effects are produeed. The section of the anterior root deprives the part supplied by the nerve of voluntary motion, sense remaining: the section of the posterior root deprives the corresponding part of the body of sensation, voluntary motion being left.

"These experiments were made by M. Magendie, and published by him in his "Journal of Experimental Physiology." But many years earlier, Sir C. Bell had made experiments upon the spinal nerves, some aceount of which had been

* Outlines of Physiology, p. 249. Fourth edition, 1837.

printed and circulated among his friends, as well as delivered in his lectures. The following is an extract from this account:—'On laying bare the roots of the spinal nerves,' observes Sir C. Bell, 'I found that I could cut across the posterior fasciculus of nerves, which took its origin from the posterior portion of the spinal marrow, without convulsing the muscles of the back; but that on touching the anterior fasciculus with the point of the knife, the muscles of the back were immediately convulsed.' Sir C. Bell was carried by these experiments very near to the truth, but he failed at that time to ascertain it; he inferred from his experiments, indeed, that the anterior and posterior roots of the spinal nerves have different functions; but in the nature of these functions he was mistaken. *Upon the anterior root he supposed both motion and sensation to depend: the posterior root he considered an unconscious nerve, the office of which was to control the growth and sympathies of parts.* Before Sir C. Bell published any other account of the functions of these nerves, M. Magendie had given to the world the true theory of their uses."

So far it is easy to recognise the identity between the statements formerly referred to, and that in the "Outlines of Physiology." But it remains to shew, how Mr. Mayo's own name has been connected with the subject, associated, as we have perceived, with that of M. Magendie.

In order to explain this point, it requires to be understood, that, in a subsequent part of his work—after having, as it would appear, dismissed the question of priority of discovery with regard to the spinal nerves, and given the credit of it to M. Magendie exclusively—Mr. Mayo returns to the subject, and alleges that

he himself, before being acquainted with the French physiologist's researches, had made the same observations respecting their roots; that is, discovered that the anterior gave motion, and the posterior sensation.

The following is the mode in which he explains his own course of proceeding in arriving at these results. Instead of going at once, like M. Magendie, to the roots of the spinal nerves, and commencing his experiments with these nerves, he (Mr. Mayo) advanced to the inquiry in a more indirect manner. When he was still unacquainted — so he represents — with the researches of that gentleman (he omits saying any thing of Sir Charles Bell or Mr. John Shaw), and while pursuing his own unassisted investigations, he was induced to examine the nerves of the brain. He first took up, he says, the portio dura and the fifth pair, and observed, with especial interest, the distinct mode in which they arose respectively from the brain. The former of them, which has a single root, he ascertained, as he states, to be limited to giving muscular power; while the latter, which has two roots, he discovered to be capable of conferring muscular power by one root, and sensation by the other. Now, it was after having established, by reasoning and experiment — so he expresses himself — these important facts relative to the roots of these cerebral nerves, that he was led, in the next place, to turn his attention to the roots of the spinal nerves, so as eventually to discover the distinct offices of each of their roots. But I prefer introducing his own words:—

"By the experiments and reasoning which I have de-

scribed," Mr. Mayo observes, "I thus established that the ganglionless portion of the fifth, and the hard portion of the seventh nerve (portio dura), are voluntary nerves to parts which receive sentient nerves from the larger or ganglionic portion of the fifth. *This happened before the publication of M. Magendie's discovery of the parallel functions of the double roots of the spinal nerves:* and, without wishing to assert the least claim to that discovery, I will yet observe, that I was led, by the well-known anatomical analogy between the fifth and spinal nerves, to conjecture nearly what M. Magendie proved, and was indeed actually engaged in experiments to determine the point when M. Magendie's were published."

The above extract bears me out in the assertion I ventured to make,—namely, that it has been by following Mr. Mayo's authority that the misapprehensions I have described have been introduced. It is, therefore, necessary, seeing the influence which this gentleman's writings possess, and however distasteful the task may be, to direct particular attention to his statements contained in the "Outlines of Physiology."

Various reasons render it imperative in me to undertake this task; but it is sufficient to mention the principal one.

If the representation given by Mr. Mayo be admitted as correct, it follows that no confidence can be placed in the theory proposed by Sir Charles Bell to solve certain questions in the nervous system, which it is of the highest importance should be satisfactorily explained. Independently of the question, Whether the nerves of motion are distinct from those

of sensation; and, Whether this distinction depends on the difference of their origins from the brain—there are other points, of greater difficulty, which the anatomy of the nervous system clearly indicates as problems which demand a solution. This is not the fit opportunity for describing fully the nature of these questions. It is enough to mention, that in the distribution of the nerves to the various organs of the body, certain peculiarities exist which we cannot ascribe to accident;—that is to say, we observe an appropriation of nerves, proceeding from distinct parts of the brain and spinal marrow, to particular organs exclusively, which marks that the distribution is regulated by some important law. To understand, therefore, properly, the functions of the nerves, or of the subdivisions of the brain and spinal marrow with which the nerves are connected at their roots, it is necessary that we should become accurately acquainted with the cause of these peculiarities. Now, previous to either Mr. Mayo or M. Magendie commencing their inquiries, Sir Charles Bell had perceived that the questions referred to demanded an explanation; and he had also, at the same time, matured a theory which he supposed was calculated to meet and solve them. A general sketch of this theory he placed before the public at that time; and it was the same as the one to which he adheres at the present day.

But if it be represented and believed, that when his theory was propounded he was ignorant of the simple and fundamental parts of the subject,—did not know, for example, that the nerves obtained

their distinct power of bestowing motion and sensation in correspondence with their roots—who would place any reliance on his explanation of these more intricate points? That the repeated disputes, as to, Whether Sir Charles Bell, at the first, was acquainted with the fact, that the one root of the spinal nerves is subservient to motion, and the other to sensation, or that the fifth pair and portio dura enjoy motion and sensation in correspondence with their origins—have had a direct influence in causing a want of confidence in the validity of the theory, can scarcely be doubted by those who peruse the most recent works on this subject. In none of them will it be found that justice has been done to the theory. Even where the author professes to acquiesce in the explanation, it is apparent, to one who understands it properly, that the chief grounds which constitute its strength are imperfectly stated; or, where it is directly opposed, it is more clearly seen to be through a misapprehension of these points. In short, I have no hesitation in saying that the questions relating to the distribution of the nerves, which it was Sir Charles Bell's main object to explain in framing his theory of the "original" and "superadded" nerves, have not been examined by the profession with the attention which their importance demands; and this circumstance has been owing principally to the doubts so commonly entertained, in consequence of the misrepresentations of others, as to whether he was acquainted, when he first proposed it, with the elementary parts of the inquiry. The consequence of this neglect is,

that no progress has been made towards removing the difficulties referred to; for, except the explanation offered by Sir Charles Bell, it is an undeniable fact, that no other has been brought forward to meet the questions alluded to. Here, then, is my first reason for undertaking to prove, that for the account of these researches contained in Mr. Mayo's work on physiology, there is in reality no foundation.

But I must add another reason. It happens, in consequence of the particular position which Mr. Mayo occupied with regard to Sir Charles Bell at the time these discoveries were first announced, that his authority, as relates to the history of their progress, carries with it a weight which would not belong to any other individual making the same statements. When these new observations were first made known to the public, or just antecedently to that time, namely, when the researches were in progress of being carried on, in the school of Great Windmill Street, with the greatest activity and zeal, Mr. Mayo was Sir Charles Bell's pupil. From this it followed, that before these observations were properly announced to the scientific world, Mr. Mayo had enjoyed the advantage of associating, not only with Sir Charles Bell, but also with Mr. John Shaw. The dissections and experiments made by these two gentlemen to illustrate their views, he must, therefore, have had the opportunity of witnessing, or hearing discussed at lecture, together with the other pupils of the school; and he must consequently have become acquainted with the new principle that guided his teachers in their researches, as well as

with the facts deduced from their dissections and experiments prosecuted according to this new mode of investigation.

Now this part of Mr. Mayo's early history as a physiologist is not unknown to the profession. One gentleman, Dr. Marshall Hall, in his published lectures on the nervous system,* has openly stated that Mr. Mayo was originally Sir Charles Bell's "familiar pupil." In other writings, to which I shall presently refer, we find it mentioned, that Mr. Mayo, previous to the promulgation of these discoveries, had been employed by Sir Charles Bell to make preparations for the museum of Great Windmill Street, with the view of illustrating the difference in the structure of the roots of the nerves. On three different occasions, before Mr. Mayo had written a single paper on the nervous system, both Sir Charles Bell and Mr. John Shaw had referred to him by name, in such a manner as to prove that he had been admitted by these gentlemen to witness their dissections. Accordingly, when these facts are thus known to the profession, can there be any doubt why the statements which he has chosen to make respecting the investigations have been received with implicit confidence? It cannot be questioned that it has been owing to this gentleman's having been originally a pupil of Sir Charles Bell, and to its being generally known that he enjoyed, on that account, the best opportunities of knowing the true history of the discoveries, and likewise, I may add, to its being

* Lancet, February 3, 1838.

commonly felt that, unless he had been strongly assured of the correctness of what he delivered, he would have been unwilling to pass an unfavourable judgment concerning his teacher,— that his representations, however unsupported by proofs or arguments, have been readily accepted by the profession as infallible.

But it is incumbent on me to shew, that, notwithstanding the opportunities he enjoyed, Mr. Mayo has presented a history of these discoveries that in every essential and material point is utterly incorrect.

To take an example, where the question relates to the fundamental principle of the researches. He assumes to himself the credit of having been the first physiologist to inculcate the importance, in investigating the functions of the nerves of the brain, of directing attention to their *roots*. Thus he represents, that it was by observing the contrast between the roots of the fifth pair and of the portio dura, that he was originally led to ascertain the distinct uses of these two nerves, and after that, to determine those of the spinal nerves. He alleges further, that his investigations on this subject were completed previous to the publication of M. Magendie's experiments on the roots of the spinal nerves.

Now M. Magendie's memoir to which he alludes, as we know by referring to its date, was communicated to the public in August 1822. Accordingly, Mr. Mayo's statement concerning his own researches is tantamount to his asserting that they were submitted to the public before that month. This view, I may remark, is also favoured by his giving the date of his " Anatomical

and Physiological Commentaries," the work which contained the account of his first inquiries into the nervous system, on the only occasion in which he refers to it*—as 1822, without particularising the month of that year.

But when we examine the real date of Mr. Mayo's first publication, we shall not find that it preceded M. Magendie's memoir. We shall discover, on the contrary, that it consisted of two distinct numbers, published at different periods; and that neither of these was given to the public before the memoir of the French physiologist, but that the first appeared in the same month as M. Magendie's paper, namely, August 1822; and the second did not appear till eleven months later, that is, July 1823.

It has, therefore, to be asked, Did Mr. Mayo, in his *first* paper, which was published simultaneously with M. Magendie's memoir, present the same views concerning the roots of the fifth pair, portio dura, and spinal nerves, that he asserts, in the "Outlines of Physiology," were matured previous to the French physiologist publishing his experiments? If, before M. Magendie related his experiments, Mr. Mayo had known the importance, in investigating the functions of the nerves, of attending to their roots, and had performed his experiments on the roots of the spinal nerves, was it not to be expected that, when he published a paper on the nerves, in the same month as that physiologist, he would have given an account of the result of these

* See p. 174. Fourth Edition.

researches, or at least have shewn some indications of his being acquainted with the proper mode of prosecuting the inquiries, in that paper?

But what are the facts? It was not till he wrote the second number of his "Anatomical and Physiological Commentaries," that he introduced any observations whatsoever on the roots of the nerves, or adverted to the experiments on the roots of the spinal nerves! It was not till July 1823, eleven months subsequent to the appearance of M. Magendie's memoir, that he touched upon these important parts of the anatomy. In his first paper, although it treated avowedly of the functions of the fifth pair, portio dura, and spinal nerves, there is not a word about the necessity of attending particularly to their origins. The experiments on the roots of the spinal nerves, of which he must have read the account, first, in Sir Charles Bell's original "Essay on the Brain" (printed in 1811), and secondly, in Mr. John Shaw's paper on "Partial Paralysis," read before the Medico-Chirurgical Society (April 1822), and which, if he did not see performed, he must have heard frequently discussed at lecture in the school of Great Windmill Street,—he altogether passed over without any notice; and as for the roots of the fifth pair and portio dura, he attached no more value to them than he did to the experiments on the roots of the spinal nerves. It so happened, that he considered himself called upon, in that first paper, to give a minute description of the anatomy of the fifth pair and portio dura. Now, in pursuing his account of them, he dwelt carefully upon the branches which

they respectively give off, and noted all the unimportant communicating filaments between them; but he never mentioned the structure of their roots! That the fifth pair arises by two roots, while the portio dura arises only by one, was not so much as hinted at in any part of his first memoir.

In short, there is, in his first paper, the most abundant evidence to prove that, notwithstanding the opportunities he enjoyed, as a pupil of Sir Charles Bell, of knowing, in investigating the functions of the nerves, the importance of attending to their roots, yet he was profoundly ignorant, when he composed that paper, of such being the true mode of prosecuting these inquiries. It was not, I repeat, till he wrote his second memoir, when, in the meantime, numerous additional papers, not only by Sir Charles Bell and Mr. John Shaw, but by other physiologists abroad and at home, had been communicated to the public, for the express purpose of recommending and enforcing by new illustrations this mode of investigation,—that Mr. Mayo, for the first time, became aware of its value. Nevertheless, and I promise to prove what I have stated in an unquestionable manner, he has confounded, in his "Outlines of Physiology," the contents of the two numbers of his "Commentaries." He has kept out of sight what was the true nature of the first memoir, and caused it to be believed, that he had published in 1822, before M. Magendie had communicated his paper in that year, what in fact he did not publish till July 1823.

These reasons, I submit, are sufficient to shew that,

in regard to the history of the researches in question, Mr. Mayo's authority does not deserve the weight which, owing to his previous connexion with Sir Charles Bell, the public is disposed to attach to it. They prove, on the contrary, the necessity, in justice to the gentlemen to whom we are really indebted for the improvements now so much prized, of giving an entirely different account of the investigations.

In doing this, I propose to treat of the subject in the following order :—

First, I will take up Sir Charles Bell and Mr. John Shaw's joint works, published before either Mr. Mayo or M. Magendie commenced writing on the functions of the nerves ; and point out what had been accomplished by them at that period. For this purpose, I shall have to bring before the reader no less than six different essays and memoirs, to which both Mr. Mayo and M. Magendie might have had access, previous to their first entering on these inquiries.

Secondly, I will give an accurate review of Mr. Mayo's first paper, contained in the first number of his "Anatomical and Physiological Commentaries."

Thirdly, I will present a similar review of his second paper, published in the second number of his "Commentaries," contrasting the contents of the two papers.

Fourthly, having exhibited the true characters of his two early works, I will direct attention to the representation of these inquiries, contained in his "Outlines of Physiology," published six years after his former papers.

Lastly, as Mr. Mayo, independently of putting forward his own claims for priority of discovery, espouses those advanced by M. Magendie, I shall be obliged to devote some observations to the labours of this French physiologist. These will be directed, not only to refute the statements in his favour made by Mr. Mayo, but to clear away some remarkable misconceptions that are generally prevalent as to the opinions originally advocated by M. Magendie in his first memoirs on the nerves.

CHAPTER I.

REVIEW OF MEMOIRS ON THE NERVES, PUBLISHED BY SIR CHARLES BELL AND MR. JOHN SHAW, ANTECEDENTLY TO MR. MAYO OR M. MAGENDIE COMMENCING THEIR INQUIRIES.

According to the plan just laid down, it is intended, in the present chapter, to give a sketch of all that was accomplished by Sir Charles Bell and Mr. John Shaw in these researches, previous to any one else joining in the same inquiries. As it was not till August 1822, that either Mr. Mayo or M. Magendie commenced publishing on the subject, I shall have to trace the labours of the two former gentlemen down to that point of time.

It has already been explained what constitutes the great and leading feature of the discoveries under discussion. It has been shewn that it consists in having established the general law,—that the brain and spinal marrow are composed of different divisions, each possessing distinct and appropriate endowments; and that the nerves, according as they originate from these different divisions, enjoy corresponding, distinct, and appropriate functions.

Now, it was for the express object of explaining this new view of the functions of the brain and nerves, that Sir Charles Bell, so far back as 1811, composed his essay, entitled, "An Idea of a New Anatomy of the Brain."

This essay, it has to be mentioned, was not published; but was merely printed for private distribution: and the reason why it was not published, is expressed on the title-page. The work was composed, it is there stated, in order that the original views which it contained might be previously "submitted to the observations of the author's friends:" or, as Sir Charles Bell, in another work, explains his intention: "After having been engaged," he says, "in the investigation of the nervous system for some years, and finding interminable labour before me, I thought of taking the opinions of my friends, and of exposing my notions to their criticisms, lest I should uselessly expend all my leisure on an unprofitable subject. It was with this purpose that I printed a little work, in 1811, entitled," &c.; "I do not know," he adds, "that I could introduce the subject with a more appropriate preface, after twenty years' familiarity with the investigations, than is in that little work. The following passage will shew how early and how uniformly I have held the same language, and how happily my anticipations have been fulfilled."*

With the reader's permission, I will proceed to lay before him some extracts from this essay, for the purpose of confirming what has been stated above. The first quotation will embrace the whole of the introductory part of the work:—

"The prevailing doctrine of the anatomical schools is, that the whole brain is a common sensorium; that the extremities of the nerves are organised so that each is fitted to

* On the Nervous System, 1830, p. 15.

receive a peculiar impression; or that they are distinguished from each other only by delicacy of structure, and by a corresponding delicacy of sensations: that the nerve of the eye, for example, differs from the nerves of touch only in the degree of its sensibility. It is imagined that impressions, thus differing in kind, are carried along the nerves to the sensorium, and presented to the mind; and that the mind, by the same nerves which receive sensation, sends out the mandate of the will to the moving parts of the body.

"It is further imagined that there is a set of nerves called vital nerves, which are less strictly connected with the sensorium, or which have upon their roots knots, cutting off the course of sensation, and thereby excluding the vital motions from the government of the will.

"This appears sufficiently simple and consistent, until we begin to examine anatomically the structure of the brain and the course of the nerves,—then all is confusion; the divisions and subdivisions of the brain, the circuitous course of nerves, their intricate connexions, their separation and reunion, are puzzling in the last degree, and are indeed considered as things inscrutable. Thus it is, that he who knows the parts the best is most in a maze; and he who knows the least of anatomy sees least inconsistency in the commonly received opinion.

"In opposition to these opinions, I have to offer reasons for believing, that the cerebrum and cerebellum are different in function as in form; that the parts of the cerebrum have different functions; and that the nerves which we trace in the body are not single nerves possessing various powers, but bundles of different nerves, whose filaments are united for the convenience of distribution, but which are distinct in office, as they are in origin, from the brain.

"That the external organs of the senses have the matter

of the nerves adapted to receive certain impressions, while the corresponding organs of the brain are put in activity by the external excitement; that the idea or perception is according to the part of the brain to which the nerve is attached; and that each organ has a certain limited number of changes to be wrought upon it by the external impression; that the nerves of sense, the nerves of motion, and the vital nerves, are distinct through their whole course, though they seem sometimes united in one bundle; and that they depend for their attributes on the organs of the brain to which they are severally attached.

"The view which I have to present, will serve to shew why there are divisions and many distinct parts in the brain; why some nerves are simple in their origin and distribution, and others intricate beyond description. It will explain the apparently accidental connexion between the twigs of nerves; it will do away with the difficulty of conceiving how sensation and volition should be the operation of the same nerve at the same moment; it will shew how a nerve may lose one property and retain another; and it will give an interest to the labours of the anatomist in tracing the nerves."—(Pp. 4-6.)

The next extracts are from the body of the essay, and tend to illustrate the same principle; namely, that for a nerve to enjoy more than one function, its roots must be connected with corresponding distinct portions of the brain or spinal marrow: the argument, throughout the whole work, being, that the brain is composed of different organs, each of which is the seat or centre of a peculiar and appropriate endowment:—

"The brain is a mass of soft matter, in part, of a white colour, and generally striated; in part, of a grey or cineritious colour, having no fibrous appearance. It has grand divisions and subdivisions; and as the forms exist before the solid bone encloses the brain, and as the distinctions of parts are equally observable in animals whose brain is surrounded with fluid, they evidently are not accidental, but are a consequence of internal structure: or, in other words, they have a correspondence with distinctions in the uses of the parts of the brain."—(P. 17.)

The next passage shews that the brain, being supposed to consist of parts endowed with distinct functions, the nerves proceeding from these parts were considered to have corresponding distinct functions. The nerves of the senses, it is also perceived, are represented as *entering* the brain, while the nerves of motion are represented as *passing out* from the brain:—

"The cerebrum I consider as the grand organ by which the mind is united to the body; into it all the nerves from the external organs of the senses enter, and from it all the nerves which are the agents of the will pass out. If this be not at once obvious, it proceeds only from the circumstance that the nerves take their origin from the different parts of the brain; and while those nerves are considered as simple cords, this circumstance stands opposed to the conclusion which otherwise would be drawn. A nerve having several roots, implies that it propagates its sensation to the brain generally. But when we find that the several roots are distinct in their endowments, and are, in respect to office, distinct nerves, then the conclusion is unavoidable that the portions of the brain are distinct organs of different functions.

To arrive at any understanding of the internal parts of the cerebrum, we must keep in view the relation of the nerves, and must class and distinguish the nerves, and follow them into its substance. If all ideas originate in the mind from external impulse, how can we better investigate the structure of the brain than by following the nerves, which are the means of communication between the brain and the outward organs of the senses?"—(P. 28.)

The following series of extracts prove that the distinctions between the senses—seeing, hearing, tasting, &c.—do not result from the construction of the outward organs; but depend on the different endowments of the nerves appropriated to each organ, and on the parts of the brain into which these nerves enter, having distinct and corresponding functions: thus confirming the main argument, that the brain, instead of consisting of parts possessing common functions, is composed of numerous organs, ministering to distinct offices:—

"In this inquiry, it is most essential to observe, that while each organ of sense is provided with a capacity of receiving certain changes to be played upon it, as it were, yet each is utterly incapable of receiving the impressions destined for another organ of sensation. It is also very remarkable, that an impression made on two different nerves of sense, though with the same instrument, will produce two distinct sensations; and the ideas resulting will only have relation to the organ affected. As the announcing of these facts forms a natural introduction to the anatomy of the brain, which I am about to deliver, I shall state them more fully.

"There are four kinds of papillæ on the tongue; but with two of these only have we to do at present. Of these, the papillæ of one kind form the seat of the sense of taste; the other papillæ (more numerous and smaller) resemble the extremities of the nerves in the common skin, and are the organs of touch in the tongue. When I take a sharp steel point and touch one of these papillæ, I feel the sharpness. The sense of touch informs me of the shape of the instrument. When I touch a papilla of taste, I have no sensation similar to the former. I do not know that a point touches the tongue; but I am sensible of a metallic taste, and the sensation passes backward on the tongue.

"In the operation of couching the cataract, the pain of piercing the retina with a needle is not so great as that which proceeds from a grain of sand under the eyelid. And although the derangement of the stomach sometimes marks the injury of an organ so delicate, yet the pain is occasioned by piercing the outward coat, not by the affection of the expanded nerve of vision. If the sensation of light were conveyed to us by the retina — the organ of vision — in consequence of that organ being as much more sensible than the surface of the body, as the impression of light is more delicate than that pressure which gives us the sense of touch, what would be the feelings of a man subjected to an operation, in which a needle was pushed through the nerve? — Life could not bear so great a pain. But there is an occurrence during this operation on the eye which will direct us to the truth. When the needle pierces the eye, the patient has the sensation of a spark of fire before the eye. This fact is corroborated by experiments made on the eye. When the eye-ball is pressed on the side, we perceive variously coloured light. Indeed, the mere effect of a blow upon the head might inform us that sensation depends on the

exercise of the organ affected, not on the impression conveyed to the external organ; for by the vibration caused by the blow, the ears ring, and the eye flashes light, while there is neither light nor sound present.

"It may be said, that there is here no proof of the sensation being in the brain more than in the external organ of sense. But when the nerve of a stump is touched, the pain is as if in the amputated extremity. If it be still said that this is no proper example of a peculiar sense existing without its external organ, I offer the following example: Quando penis glandem exêdit ulcus, ut nihil nisi granulatio maneat, ad extremam tamen nervi pudici partem, ubi terminatur, sensus supersunt."—(P. 12.)

"The nerves of sense—the olfactory, the optic, the auditory, and the gustatory nerves—are traced backwards into certain tubercles, or convex bodies, in the base of the brain. And I may say, that the nerves of sense either form tubercles before entering the brain, or they enter into those convexities in the base of the cerebrum. These convexities are the constituent parts of the cerebrum, and are in all animals necessary parts of the organs of sense; for as certainly as we discover an animal to have an external organ of sense, we find also a medullary tubercle; whilst the superiority of animals in intelligence is shewn by the greater magnitude of the hemispheres, or upper part of the cerebrum."—(P. 29.)

In the next extracts, the same train of observation is pursued as in one of the preceding; namely, that the nerves of sense convey their influence inwardly, to appropriate organs in the brain, while those of motion convey theirs outwardly, from a distinct part; and that the various nerves, transmitting these different properties to and from the brain, are enveloped, in

their course through the body, in the same cellular sheath :—

"The convex bodies which are seated in the lower part of the cerebrum, and into which the nerves of sense enter, have extensive connexion with the hemispheres on their upper part. From the medullary matter of the hemispheres, again, there pass down, converging to the crura, striæ, which is the medullary matter, taking upon it the character of a nerve; for from the crura cerebri, or its prolongation in the anterior fasciculi of the spinal marrow, go off the nerves of motion.

"But with these nerves of motion which are passing outward, there are nerves going inwards—nerves from the surfaces of the body, nerves of touch, and nerves of peculiar sensibility, having their seat in the body or viscera. It is not improbable that the tracts of cineritious matter, which we observe in the course of the medullary matter of the brain, are the seat of such peculiar sensibilities; the organs of certain powers which seem resident in the body."—(P. 30.)

The following extracts prove how fully Sir Charles Bell was acquainted with the fact, that sensation is not a common property belonging to all parts of the nervous system. The nerve of vision, for example, conveys a sense from external objects inwardly to the brain; but it is incapable of bestowing the same kind of impression as a nerve of touch: it is confined to giving impressions of the diversities of light, and will not excite pain upon injury. In like manner, although certain parts of the brain manifest sensibility, it is not to be expected that parts of the organ which have a

different office to perform from that of receiving common sensation, should exhibit that property :—

"As we proceed further in the investigation of the function of the brain, the discussion becomes more hypothetical. But surely physiologists have been mistaken in supposing it necessary to prove sensibility in those parts of the brain, which they are to suppose the seat of the intellectual operations. We are not to expect the same phenomena to result from the cutting or tearing the brain as from the injury to the nerves. The function of the one is to transmit sensation; the other has a higher operation. The nature of the organs of sense is different; the sensibilities of the various parts of the body are very various. If the needle, piercing the retina during the operation of couching, gives no remarkable pain except in touching the common coats of the eye, ought we to imagine that the seat of the higher operations of the mind should, when injured, exhibit the same effects with the irritation of a nerve? So far, therefore, from thinking the parts of the brain which are insensible to be parts inferior (as every part has its use), I should even from this be led to imagine that they had a higher office. And if there be certain parts of the brain which are insensible, and other parts which, being injured, shake the animal with convulsions, exhibiting phenomena similar to those of a wounded nerve, it seems to follow that the latter parts which are endowed with sensibility like the nerves, are similar to them in function and use; while the parts of the brain which possess no such sensibility are different in function and organisation from the nerves, and have a distinct and higher operation to perform.

"At first, it is difficult to comprehend how the part to which every sensation is referred, and by means of which

we become acquainted with the various sensations, can itself be insensible; but the consideration of the wide difference of function betwixt a part destined to receive impressions, and a part which is the seat of the intellect, reconciles us to the phenomenon. It would be rather strange to find that there were no distinctions exhibited in experiments on parts evidently so different in function as the organs of the senses, the nerves, and the brain."—(P. 34.)

I will close the extracts by inserting the account of the experiments which the author performed on the roots of the spinal nerves:—

"In thinking of this subject, it is natural to expect that we should be able to put the matter to the test of experiment. But how is this to be accomplished, since any experiment direct upon the brain itself must be difficult, if not impossible? I took this view of the subject: the medulla spinalis has a central division, and also a distinction into anterior and posterior fasciculi, corresponding with the anterior and posterior portions of the brain. Further, we can trace down the crura of the cerebrum into the anterior fasciculus of the spinal marrow, and the crura of the cerebellum into the posterior fasciculus. I thought that here I might have an opportunity of touching the cerebellum, as it were, through the posterior portion of the spinal marrow, and the cerebrum through the anterior portion. To this end I made experiments, which, though they were not conclusive, encouraged me in the view I had taken.

"I found that injury done to the anterior portion of the spinal marrow convulsed the animal more certainly than injury done to the posterior portion; but I found it difficult to make the experiment without injuring both portions.

" Next, considering that the spinal nerves have a double root, and being of opinion that the properties of the nerves are derived from their connexions with the parts of the brain, I thought that I had an opportunity of putting my opinion to the test of experiment, and of proving, at the same time, that nerves of different endowments were in the same chord, and held together by the same sheath.

" On laying bare the roots of the spinal nerves, I found that I could cut across the posterior fasciculus of nerves, which took its origin from the posterior portion of the spinal marrow, without convulsing the muscles of the back; but that, on touching the anterior fasciculus with the point of the knife, the muscles of the back were immediately convulsed.

" Such were my reasons for concluding that the cerebrum and the cerebellum were parts distinct in function, and that every nerve possessing a double function obtained that by having a double root. I now saw the meaning of the double connexion of the nerves with the spinal marrow, and also the cause of the seeming intricacy in the connexion of nerves throughout their course, which were not double at their origins.

" The spinal nerves being double, and having their roots in the spinal marrow, of which a portion comes from the cerebrum, and a portion from the cerebellum, they convey the attributes of both grand divisions of the brain to every part; and therefore the distribution of such nerves is simple, one nerve supplying its destined part. But the nerves which come directly from the brain, come from parts of the brain which vary in operation; and, in order to bestow different qualities on the parts to which the nerves are distributed, two or more nerves must be united in their course, or at their final destination.

" Hence it is that the first nerve must have branches of

the fifth united with it; hence the portio dura of the seventh pervades every where the bones of the cranium to unite with the extended branches of the fifth: hence the union of the third and fifth in the orbit: hence the ninth and fifth are both sent to the tongue: hence it is, in short, that no part is sufficiently supplied by one single nerve, unless that nerve be a nerve of the spinal marrow, and have a double root, a connexion (however remotely) with both the cerebrum and cerebellum.

"Such nerves as are single in their origin from the spinal marrow, will be found either to unite in their course with some other nerves, or to be such as are acknowledged to be peculiar in their operation."—(P. 21.)

"Understanding the origin of the nerves in the brain to be the source of their powers, we look upon the connexions formed betwixt distant nerves, and upon the combinations of nerves in their passage, with some interest; for without this the whole is an unmeaning tissue. Seeing the seeming irregularity in one subject, we say it is accident; but, finding the connexions never vary, we say only that it is strange, until we come to understand the necessity of nerves being combined in order to bestow distinct qualities on the parts to which they are sent."—(P. 25.)

After reading the above extracts, no one can refuse to admit that, in this early work, the important principle, which has given a new feature to the researches in the nervous system, was clearly and explicitly laid down. It will also be allowed, that the illustrations contained in it were sufficient, even if nothing else had been subsequently done, to confirm the truth of this principle. In short, to establish that the subdivisions of the brain minister respectively to

distinct offices, and that the nerves are endowed with appropriate functions in correspondence with their origins, is the main purpose of every page of the essay.

Again, it will have been observed how frequently the distinction between the nerves of motion and sensation is referred to. These functions are represented as of such a contrary nature to each other, that to suppose them both united in the same nerve, is treated of as next to imagining an impossibility. For example, the following train of argument is pursued. In whatever mode the nervous influence which causes the muscles to contract may be supposed to operate, of this, at least, we are certain, that it must be propagated in one particular direction alone; that is to say, the impulse (if we may so call it) which is destined to excite the muscles to contraction must travel from the brain, where it originates, *outwardly* to the muscles, which have to be drawn into action. On the other hand, whatever may be the nature of the nervous influence which causes a sensation, it must of necessity proceed in a directly opposite course from the former; that is to say, as the impression which the nerves of sensation are destined to carry, has to be communicated from the integument to the sensorium, and the remote extremities of the nerves distributed on the skin are the first affected, the impulse must be conveyed *inwardly* to the sensorium. Hence, unless we are to suppose that a single nerve can propagate one nervous influence outwardly from the brain, and another inwardly towards the brain simultaneously, we must consider it

against all reason to admit that motion and sensation can reside together in the same nerve. Such, then, was a principal argument employed by Sir Charles Bell to prove that distinct organs in the brain and spinal marrow must be provided for these two endowments.

But notwithstanding these observations it will be found, by attending to the description of the experiments on the roots of the spinal nerves (p. 41), that the author has refrained from saying directly to which of the roots *sensation* belongs. That the anterior root was alone capable of exciting the muscles to contraction, he has affirmed in sufficiently positive terms; his experiments made that clear and indisputable: but he has not attached, it would appear, that degree of confidence to his observations which would allow him to say distinctly, on their authority, on which of the roots sensation depended.

The cause of this omission is not to be understood, unless we take into consideration the peculiar nature of the experiments in question. To be convinced that, with whatever degree of caution or adroitness the experiments on the spinal nerves are performed, the greatest uncertainty must always be felt as to the particular root which confers *sensation*, let it only be recollected what is the process of exposing these roots in a living animal. The physiologist who undertakes such an experiment is obliged to perform an operation, the rudest and most cruel that was ever devised for the purposes of science. And this severe operation, it has to be added, has for its object to lay bare parts which, of all the organs of the body, are the

most liable to derangement, or destruction of their functions by concussion and external injury. He has, in the first place, to cut extensively through the skin; then he must carry his knife through several successive layers of thick and tendinous muscles: after that, he has to apply his saw to a chain of irregularly formed bones; and having divided these bones, he must introduce his levers and bone scissors into the interior of the vertebral canal, to tear and break up the fragments, and disclose the parts contained within. Now, can it be supposed that, after suffering from the tortures of such a proceeding, there is any animal, however submissive to the infliction of pain or high in its courage, that could endure the further and concluding parts of the experiment with such a degree of patience as to admit of correct observations being made in regard to the amount of sensibility appertaining to either of the roots? When the membrane investing the spinal marrow has been slit up and the roots displayed, can it be supposed that the animal is in such a condition as to enable us to judge satisfactorily whether its struggles and cries result from the severity of the wound inflicted, or depend on the fresh injuries that we commit on the roots of its nerves?

Such objections may justly be brought against experiments designed to settle the question as to which of the roots of the spinal nerves confers *sensation;* but it is different with regard to the particular root which bestows muscular power. In this case it signifies nothing whether, in breaking up the vertebræ, the spinal marrow between the seat of the experiment

and the brain has been injured or not. It is not even of much consequence whether the animal has survived the operation, if the roots be quickly exposed after death. Each time that the anterior roots are touched with the point of the knife or probe, an immediate and visible effect is produced. A convulsion of the muscles, obvious to the eye and felt with the hand, so as to leave no doubt as to the cause which produced it, follows each successive touch of the instrument; and no similar effect can be obtained by irritating the posterior roots.

Accordingly, that it was left in doubt by Sir Charles Bell, when he composed his "Essay on the Brain," in 1811, whether the power of giving sensation belonged to the posterior root, must be admitted.* This power of conveying sensation, he was aware, must reside in a different part from that which confers motion; it must have a distinct seat or organ in the brain, and an appropriate set of nervous filaments; but from the experiments he performed, he was uncertain whether this power belonged to the posterior roots, or whether

* To be convinced how little we can depend on the experiments within the vertebral canal alone, for solving the question, as to which root confers sensation, we need only look to what they are doing in Paris at this very time (July 1839). M. Longet, in repeating the experiments on the spinal nerves, thinks he has discovered the fallacy which has hitherto led M. Magendie to ascribe sensation to both roots in common; for that is the real opinion entertained by this physiologist: and the subject is now undergoing discussion before the Institute, with as much lively interest as if we were at the very commencement of the inquiries. I beg the reader also to refer to Müller on this question. It is one to which I must return in a subsequent part of the work.

the anterior roots might not have filaments subservient to this function combined with them, and accompanying them in their course, so as to render these roots compound in their structure and properties; that is, consisting of some filaments which gave motion, and others which gave sensation.

But because he met with this difficulty, let it not be supposed that he was unsuccessful in ascertaining the truth, before any one else engaged in the same investigations. On the contrary, Sir Charles Bell has always affirmed, that although he could not satisfactorily determine the point in question, by confining himself to experiments within the vertebral canal; yet, by his subsequent researches on the cerebral nerves, he removed all doubts from the subject, and established, that the anterior root was subservient to motion alone, and the posterior to sensation alone.

To prove the correctness of this statement, I solicit the reader's attention to that part of his progress, in which the nerves of the brain next occupied his views.

In prosecuting this part of the subject, it cannot be questioned that one particular observation exercised a more powerful influence than any other in imparting a new interest to the inquiries, and in elucidating the functions of the spinal nerves. I allude to the observations on the anatomical structure of the fifth pair of cerebral nerves. That is to say, to its being perceived that this nerve, in originating by two roots, upon one of which a ganglion is formed, is the only nerve of the brain which bears a resemblance to the spinal nerves.

To those, I must remark, who would critically examine the history of these investigations, it is absolutely necessary to become acquainted with the state of knowledge respecting the roots of the nerves, at the period when the inquiry was first instituted. Unless some attention be paid to this subject, it will not be possible to comprehend how Sir Charles Bell, although he had undoubtedly hold of the true principle for enabling him to ascertain the functions of the nerves of the brain, should have omitted to take any notice of the structure of the roots of the fifth pair, when he composed his original essay.

It requires to be kept in mind, that before the principle on which these investigations have been founded — namely, that the functions of the various nerves depend on their distinct roots—was duly laid before the profession, the anatomy of these parts attracted little or no general attention. No stronger evidence of this need be given, than to refer to the fact that it was reserved for the second Munro* to make the discovery, that it is on the posterior roots of the spinal nerves alone that the ganglions are formed. Yet this remarkable distinction, important as we now consider it, was not viewed with much interest at first. If we refer to works of excellent authority, published subsequently to Munro's treatise, we shall find the anatomical difference between the two roots as frequently omitted in giving the description of the spinal nerves, as pointed out. Again, with regard to the nerves of

* Engravings of the Nerves, pp. 30–50.—1783.

the brain; it was not till the time of Wrisberg, that the true nature of the origin of the fifth pair began to be understood. After this anatomist had drawn attention to the fact, that the fifth nerve consists at its origin of two roots, upon the larger of which a ganglion is formed, it has to be admitted that various other writers dwelt upon the same subject. Hence, in the works of Paletta, Prochaska, Niemeyer, Soemerring, very complete views of the anatomy of the nerve are to be discovered. Nevertheless, other anatomists, of still higher reputation than those I have named, omitted altogether to advert to the double roots of the fifth, when treating of its minute anatomy. For example: Meckel composed an elaborate memoir on the nerve,* and yet he nowhere adverted to its arising from the brain by two distinct roots. Then Munro, in his "Engravings of the Nerves," presented four different views of the Gasserian ganglion, formed upon the root of the fifth; but he omitted either to represent the lesser root, which passes by the ganglion, or to speak of it in the text of his work. The following sentence bears upon the question, and I may therefore introduce it:—"That ganglia," Munro remarks, "do not serve to render motions independent of our will, as an ingenious writer has supposed, is evident, without observing more than that *all* the branches of the fifth pair, and the posterior half of all spinal nerves of the voluntary muscles, pass through ganglia."† Lastly, Mr. John Hunter had occasion to give

* Ludwig. † Loc. cit. p. 58.

a description of this nerve in presenting his discovery of an unobserved branch belonging to the ophthalmic division; yet he made the same omission as the others to whom I have referred, with regard to the roots. In short, I may observe that, after a careful search, I have not succeeded in finding a single work in the English language, in which any allusion is made to the true structure of the origin of this nerve, before Sir Charles Bell commenced his investigations.

Now it was in this state of our knowledge with regard to the anatomy of the roots of the nerves, and as it will afterwards appear, when labouring under the disadvantage of being unacquainted with the best treatises on the subject, that Sir Charles Bell had his attention drawn to the peculiarities in the anatomical structure of the fifth pair to which I have referred.

As I have already had occasion, in the Introduction (see p. 9), to explain the general functions of this nerve, and also of the portio dura, when shewing how the examples of these two cerebral nerves were brought forward by Mr. John Shaw, to confirm his statements concerning the roots of the spinal nerves, it may appear superfluous to describe again the views which were formed concerning them. The fifth pair, I must, however, repeat, in consideration of its being the sole nerve of the brain which arises by double roots, analogous to those of the spinal nerves, was concluded to be the only one possessed of the same kind of functions as these nerves; that is, it was shewn to give motion in correspondence with the distribution of one of its roots,

and sensation in correspondence with the distribution of the other, and was pronounced to be, the "nerve of sensation and mastication." With regard to the portio dura, on the other hand, as it differed essentially in its origin from the spinal nerves — arising by a single root instead of by two — it was represented as having only one of the functions possessed by the spinal nerves and fifth pair; in other words, to be provided for bestowing motor power alone.

To shew how fully the functions of these two cerebral nerves had been pointed out at this time, I will select, in the first place, one or two quotations from the "Manual of Anatomy," by Mr. John Shaw, a work published in September 1821; that is, eleven months before either Mr. Mayo or M. Magendie composed their first papers on the nervous system. I may also remark, that as the experiments on the lower animals, by which these views had been originally verified, had been performed for some time previously, opportunities had occurred in the interval, of comparing their results with the appearances presented in patients who had suffered from injuries or diseases of these nerves. In place, therefore, of taking my examples from the experiments on brutes, I will bring forward some of the cases that were related as illustrations of the effects produced by destruction of the nerves in question, in man. The first relates to a disease of the portio dura: —

"Since the use," Mr. Shaw observes, "of the portio dura has been illustrated by the facts of comparative anatomy,

and by the various experiments instituted by Mr. Bell, we have been able to explain many symptoms of disease which have hitherto been misunderstood by surgeons. That I may direct the student's attention more particularly to this subject, I shall mention one or two cases which are illustrative of the consequence of an injury to this nerve. In a case of cynanche parotidea, when suppuration took place, every muscle to which the portio dura went was paralysed in the act of respiration or expression, but the same muscles were still efficient in the act of mastication: thus when the patient attempted to whistle, or when he was made to sneeze, the muscles of only one side acted; but when he chewed his food, the muscles of both sides were in full action. This paralysis continued for a considerable time after the sinuses were healed. I then lost sight of the patient.

"I was lately consulted in a very interesting case, nearly of a similar nature. A young lady had, for several years, a distinct twist of one side of her mouth, particularly when she smiled; but of late she had an affection of her eyelid. As she was under the care of a gentleman who was acquainted with the experiments which we had been making in Windmill Street, the cause of the twist of the mouth was by him correctly referred to a severe attack of inflammation [in a gland below the ear], which the lady had had some years ago: but as he found it difficult to understand why the eye should be also now affected, he begged that I would see the patient with him.

"On noticing the action of the muscles, which I did when the lady was sitting at luncheon, I observed that no action was deficient while she was eating; but that there was a distinct paralysis when she smiled or laughed: however, I was a little puzzled to see the muscles of the mouth so

distinctly affected and not those of the eye, because I had found in all the experiments in which the portio dura was cut, and in the cases where the paralysis had been produced by an inflamed gland under the ear, that both the muscles of the eye and of the mouth were affected at the same moment. But, on further inquiry, the cause of the difference in this case was explained; for I found that the inflammation, which had been the original source of the injury to the nerve, was confined to the space below the molar teeth, so that the branches of the nerve which go to the muscles round the eye were not included in the disease.

"There are certain tumours under the ear, which are of so dangerous a nature that it is necessary to remove them without taking into account the paralysis consequent upon cutting the branches of the portio dura; still there may be cases where the patient will not thank the surgeon for ridding him of a trifling tumour at the expense of having ever after distortion of the face. Very lately, a gentleman wished me to cut out a small harmless tumour which was situated immediately upon the branch of the nerve which goes to the side of the mouth; but, on putting the question to him, whether he would run the risk of having the side of his mouth paralysed, or retain the small tumour, which might almost be concealed by his whiskers, he chose to submit to the disfigurement produced by the tumour, as probably the lesser of the two.

"I think it hardly possible for surgeons now to propose to cut the branches of the fifth pair, or portio dura, indiscriminately, for the disease called the tic-douloureux. There is reason to believe that the disease is seldom or never in the portio dura; and the question of the propriety of cutting the fifth is very doubtful."—(P. 290.)

The observations on the above cases make it evident, that Mr. John Shaw was acquainted with the fact that the portio dura was confined to giving motion alone; while the fifth pair was the nerve of sensation of the head, and conferred motion besides on a certain class of muscles. The next quotations are from this gentleman's paper entitled "On the Effects produced on the Human Countenance by Paralysis of the different Systems of Facial Nerves." This was communicated to the "Quarterly Journal of Science," in the month of March 1822.

"*Case of complete paralysis of those actions of the muscles of the face, which are regulated by one of the nerves of the superadded or additional system; viz. the portio dura, or respiratory nerve of the face.*

"Rebecca Larkin, aged twelve, daughter of a marble-cutter, in Cirencester Place.—This is a pretty little girl, in full health; and, according to her mother's account, a good scholar and an adept at her needle. . . . When she laughs heartily, the right side of the cheek and right side of the mouth are unmoved, while the muscles of the left side are convulsed with laughter. If she be told to try and laugh with the right side, she raises the angle of the mouth, but by an action which is evidently regulated by the fifth pair. This attempt to laugh gives a peculiarly droll expression to her face, and, as I have said in a former communication, is probably the same action as that with which we are so much amused in observing the face of a famous mimic. When the girl attempted to whistle, she could not close the right side of her mouth; still she could purse it up (this action of the orbicularis

muscle being regulated by the fifth), so as to hold a whistle which I put between her lips, and then by blowing she could sound the whistle; but in doing this, there was no action perceptible in the right cheek, for it was distended like part of a leathern bag, while the muscles of the left were in full action. In this experiment, it was shewn that the muscles of the right side were in a state similar to those of the cheek in an animal in which the portio dura of the same side has been divided, since their actions were paralysed so far as they depended on that nerve.

"My next inquiry was, How far those actions of the same muscles which are regulated by the fifth nerve were perfect? The action of the right buccinator, during the time the child was eating, was not only distinct, but she even preferred chewing her food upon the right side. As this was hardly to be expected, I inquired if there was any thing the matter with her teeth, which could induce her only to eat on the right side; and the girl, with much acuteness, perceiving the object of my inquiry, said she had always chewed her food on the right side, and she recollected that her grandfather used to scold her for doing so. The circumstances already detailed are sufficient to prove, that the same muscles of the mouth that were paralytic in those actions which are subservient to expression and respiration, were perfect in any voluntary action.

"The degree of sensibility in the two cheeks was exactly the same; which was the only additional circumstance necessary to shew that the fifth nerve was perfect, in all respects, on the lower part of the right side of the face. I next examined the state of the nose. When ammonia was held to her nostrils she inhaled it only with the left; the irritation caused by the fumes was followed by all the symptoms of sneezing, but the expression was confined to

the left side of the face. The left nostril was then closed, and the ammonia held to the right, but she could not snuff it up, nor was there the slightest symptom of sneezing produced, though the fumes of the ammonia were so strong as to make her eyes water. This was so similar to the experiments on the ass, that I thought it a conclusive proof of the respiratory action of the muscles of the right nostril being destroyed. To try the power of the branches of the fifth over this division of the nose, I tickled the inside of the same nostril with a feather, and then all the symptoms of being about to sneeze were produced on the opposite side of the face. On examining the state of the eye-lids, I found that the orbicularis oculi of the right side was much weaker than the left; it appeared wasted, and this probably from want of use, as she cannot exert it as she does the other, nor is there any action observed in it when she sneezes or laughs."—(P. 123.)

Here the same facts have been exhibited as in the preceding cases; namely, that the portio dura is exclusively a motor nerve; while the fifth pair, in correspondence with its arising by a double root, is both a motor and a sensitive nerve.

The following case was introduced to illustrate the effects of an injury implicating both the portio dura and a part of the fifth pair, after they had escaped from the skull; the lesion of the former nerve producing loss of motion alone; that of the fifth, the loss of motion and also of sensation.

And here I may be allowed to offer a remark on the importance of these observations, in relation to practice. Formerly, when it was conceived that

every nerve could give motion and sensation equally, and it happened that any part of the body was deprived of either of these powers, the conclusion was drawn that the paralysis resulted from disease in the brain, and it was supposed to threaten an attack of apoplexy. But now that the distinct offices of the individual nerves have been ascertained, we are enabled to trace the loss of power, whether of motion or sensation, to the particular nerve affected. Consequently, we can distinguish a disease which produces what appear to be formidable symptoms, by merely involving a nerve, after its escape from the skull, from another situated within the brain, and of the most imminent danger to life. An example of this kind of observation is presented in the case that follows, although others, perhaps more striking, are to be found in Mr. Shaw's paper:—

"—— Phipps, a brieklayer, on the 1st of September, fell from a seaffold thirty feet high. . . . . On the 1st of Oetober, he was made an out-patient, his faee being at this time very mueh distorted. On the 17th of Oetober, I visited him at his own house, and, with the assistanee of Mr. Hawkins, examined particularly into all his symptoms. The general appearanee of his faee was that of a man who has suffered paralysis from apoplexy; but when he spoke or laughed, the distortion was mueh inereased, the month being pulled more to the left side than I ever saw in any other patient. The following are the notes that were taken at the time. There appears to be total paralysis of the muscles of the right side of the faee. When he smiles or laughs, they are passive, while those of the left are regu-

larly in action. If he attempt to whistle, he cannot close his lips sufficiently; when he blows, the right cheek is dilated, but passive like a distended bladder. He can smoke by putting the pipe into the left side of his mouth; he throws the smoke out of the right side, but in doing this, the action is all evidently in the muscles of the left cheek. These several circumstances were sufficient to prove, that those actions of the muscles of the cheek and mouth, which are regulated by the respiratory nerve (portio dura), were deficient.

"The next question was, How far those actions of the same muscles, which we conceive to be regulated by the fifth pair, were perfect? The cheek and mouth hang low, as in the common case of hemiplegia: he cannot by a voluntary action move his cheeks. When a piece of bread is put between the teeth and cheek of the right side, he cannot push it out with the buccinator, but picks it out with his tongue. He cannot hold his pipe or my pencil with his right side of his lips. Our next inquiry was into the degree of sensibility in the paralysed side. The difference in the sensibility of the two cheeks was most distinct. When I pulled a hair of the right whiskers, he was not conscious of my doing so; but he started immediately on my pulling one from the left. . . . . November 16th. He is still very deaf on this side. *State of his tongue.*—He can put it out, and move it in every direction, with the greatest ease; the motions are all apparently correct and natural: he can throw a morsel from one side of the mouth to the other, and towards the throat. These observations led us to conclude, that not only the motor linguæ, or ninth nerve, but also the glosso-pharyngeal, were perfect. We next examined the state of those parts of the tongue which are supplied by the third division of the fifth nerve.

After closing his eyes, we put a little sugar on each side; we asked him to put his finger to the side of the tongue upon which the sugar was; he put it up to the left side, he was not conscious that there was any on the right. While his eyes were closed, we pricked both sides of the tongue with a needle; his expression with regard to the difference of feeling was the same as that used in describing the state of his cheek."—(P. 132.)

The next extracts are from the same gentleman's paper on "Partial Paralysis," which, I have already had occasion to state, was read before the Medico-Chirurgical Society, April 1822, five months before Mr. Mayo or M. Magendie commenced treating of the same subjects:—

"On holding ammonia to the patient's nose (referring to a patient who had the portio dura destroyed by disease), it was observed that he could not inspire with the right nostril; and on examining the state of the muscles, when the act of sneezing was excited by the ammonia snuffed up by the left nostril, it was found that not only those of the right side of the nose and mouth, but also of the eyelids, were passive, while all the muscles of the left side were in full action. When he blew or attempted to whistle, the air escaped by the right angle of the mouth, the right buccinator not at all corresponding in action with the muscles of the left side, nor with that of the muscles of the neck and chest, by which the air was expelled.

"On exciting those actions which, by experiments on animals, we had proved to depend on the fifth pair of nerves, they were all found perfect; that is, the patient could bring the orbicularis oris into such action as to hold

a pencil with it: he could throw a piece of bread from the right to the left side of his mouth by the action of the buccinator, and he could close his jaws with equal force on both sides. The sensibility of the paralysed cheek was equal to that of the other side."—(P. 9.)

The next is also a case of paralysis from destruction of the portio dura. In this patient the nerve was destroyed by abscess forming within the ear.

* * * "A most remarkable appearance in the face of Garrity is the wasting of all the muscles of the face which are subservient to respiration and expression. His cheek is so thin, that when he speaks, it flaps about as if it were only skin; and the corrugator supercilii and occipito-frontalis, which are principally muscles of expression, are so wasted, that we might at first suppose they had been removed by operation, and that now the bones were only covered with skin. There can be little doubt that the wasting of these muscles is in consequence of their not having been called into action for many years, since the masseter and temporalis muscles of the same side are not at all diminished in size, being as large as those of the opposite side."—(P. 19.)

After reading the above extracts, no one can doubt that the only function which was ascribed to the portio dura, was the power of giving motion to a particular class of muscles. That this nerve was known to be altogether incapable of bestowing sensation, is proved by its being distinctly stated that this property belonged to the fifth pair exclusively. Again: it is to be observed that the muscular actions which

the portio dura was represented as controlling, were either of a voluntary or involuntary kind. For example, in three or more of the passages just quoted, it is shewn that the patients who had lost the function of this nerve, were deprived of the power of "whistling;" in others, that they could not "speak" distinctly; in others, that they could not command the motions of the lips in "blowing;" in one or two cases, it is mentioned that the patients could not "inhale" the fumes of ammonia held to the affected nostril; and all these actions, it cannot be doubted, are voluntary. But, on the other hand, it is admitted that the nerve could superintend certain actions decidedly involuntary. Thus it was shewn that the nostrils and mouth were deprived of the power of combining with the other parts of the organ of respiration, in common, and excited breathing; again, that the features no longer exhibited the changes produced by mental emotion; lastly, when treating of the eyelids, it was stated that they were deprived of motion, by which we must understand that they had lost not only the power of closing voluntarily, but their involuntary winking actions, and that they did not shut in sleep.

In the next place, with regard to the fifth pair: in correspondence with its arising from the brain by two distinct roots, the two separate functions — namely, of giving motion on the one hand, and sensation on the other, were assigned to it.

But the reader is requested to notice, that although no one can refuse to allow, that the account here

given of the functions of the fifth pair was perfectly correct, yet the experimental proofs by which it was intended to verify it were unfortunately insufficient. That is to say, so far as proving that the fifth nerve could bestow sensation, and was the only nerve of sensation in the head, the evidence was ample and unquestionable: but it so happened that the only experiment brought forward to establish, that besides conferring sensation, it could also regulate the muscles, was deceptive, and did not prove that fact.

To make this matter clear is of some consequence; and I may therefore be allowed to enter upon a short explanation of the subject.

It must be granted that when Sir Charles Bell commenced his experiments on the two facial nerves, his knowledge of the exact course pursued by the motor root of the fifth pair was not perfect. That he knew that of the two roots of which this nerve was composed, it was the smaller, non-ganglionic one, which gives motion, cannot be doubted. First, he was acquainted with the analogy existing between this nerve and the spinal nerves; and his experiments on the latter had satisfied him that the anterior roots, which have no ganglions, are those that bestow motion; while he inferred that the posterior or ganglionic roots give sensation. Again, the fact of his applying the name "nerve of sensation and mastication" to the fifth confirms the same view: because we know that the larger root, which has the ganglion formed upon it, ramifies over every part of the head, not supplied by the spinal nerves, bestowing sensation generally; while

the smaller, non-ganglionic root, is distributed exclusively to the muscles engaged in moving the jaws in mastication. But it is to be regretted (owing to the discussions which have arisen, as will hereafter be seen, from this cause) that on first describing the anatomy of the fifth pair, he did not give a more accurate account of the two roots.* If he had been acquainted (which it is obvious he was not) with the excellent descriptions of the nerve by the foreign anatomists, whose names I have mentioned, he would have avoided falling into the error which I am about to point out; and which error has been made use of, in an unwarrantable manner, as a handle to misrepresent his views.

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* Putting out of view his frequent statements that the fifth pair resembled the spinal nerves, the following imperfect description is the only one that he gave of this nerve.

"This nerve (the fifth) comes off from the base of the brain, in so peculiar a situation that it alone of all the nerves of the head receives roots both from the medullary process of the cerebrum and of the cerebellum. A ganglion is formed upon it, near its origin, though some of its filaments pass on without entering into the ganglion."—*Phil. Trans.*, July 1821.

When this was written, Sir Charles Bell conceived that the anterior column of the spinal marrow was a prolongation from the cerebrum; while the posterior was continuous with the cerebellum (see p. 40); and that the former presided over muscular motion, while the latter was the seat of sensation. This may explain the observation as to the fifth pair, the analogous nerve of the spinal nerves, having a connexion by its roots with both these organs. A more accurate examination of the course of the columns of the brain and spinal marrow, and of the origin of the fifth pair, has, however, led him subsequently to modify this opinion.—See *Nervous System*, 1837, p. 231.

The error, then, to which I refer was this. The lesser or motor root of the fifth pair, as will be seen by consulting the frontispiece, joins itself exclusively to the third, or inferior maxillary division: that is, it omits sending any of its filaments either to the first or second divisions. But Sir Charles Bell erroneously supposed that certain filaments of this root did join the second division, and were prolonged by this means to the infraorbitary branch—the terminating branch of the second division. The infraorbitary branch, it has to be noticed, immediately after escaping from the foramen of the same name, in the superior maxillary bone, divides into three minor sets of branches: one set passing to the lower eyelid; another to the side of the nostril; and a third to the *lips*. Now, as the lips form a part of the organ of mastication, it was conceived by Sir Charles Bell that the filaments, to which I have referred as being supposed to join the second division, were prolonged to these parts exclusively; that is to say, it was only that set of the branches of the infraorbitary nerve which goes to the lips, that he considered to be partly composed of motor filaments, as well as sensitive filaments; for there is no evidence to shew that he ever supposed the branches which pass to the lower eyelid or the nostril, to be compound. Accordingly, from thus imagining that the labial portion of the infraorbitary branch, by its anatomy, could give motion as well as sensation, he was led to select this particular branch for his experiments; and that such were his reasons for choosing it, will be admitted by every candid

person who takes the trouble to inquire carefully into the subject.

Now the results of the experiments performed upon the infraorbitary branch, were of such a nature as to deceive him, and to lead him to imagine he had obtained a confirmation of what he looked for, when such was not really the case. Upon cutting across the branch in a living animal, and leaving the portio dura, which gives branches to the same parts, entire, he observed that the nostril of the corresponding side continued to expand and contract, and the lips could be protruded and moved about in different directions as before. This corresponded with what he must have anticipated; because he was aware that the portio dura could alone control the actions of the nostrils, and that it regulated besides both the voluntary and involuntary movements of the lips (see p. 61). But he discovered that, at the same time that the animal could move its lips in various directions, voluntarily and involuntarily, they were paralysed in certain kinds of actions: that is to say, the animal could no longer gather the food in eating. When oats were placed before the ass—the animal on which the experiment was performed, it was observed that he could not pick them up. The following is the description of the experiment:—

"An ass being tied and thrown, the superior maxillary branch (infraorbitary) of the fifth nerve was exposed. Touching this nerve gave acute pain. It was divided: *but no change took place in the motion of the nostril:* the

cartilages continued to expand regularly in time with the other parts which combine in the act of respiration: but the side of the *lip* was observed to hang low, and it was dragged to the other side. The same branch of the fifth was divided on the opposite side, and the animal let loose. He could no longer *pick up his corn: the power of elevating and projecting the lip, as in gathering food, was lost. To open the lips, the animal pressed the mouth against the ground, and at length licked the oats from the ground with his tongue. The loss of motion of the lips in eating was so obvious*, that it was thought a useless cruelty to cut the other branches of the fifth."

Now, it cannot be doubted that this result, namely, the loss of motion in the lips in eating, was thought by Sir Charles Bell to depend on those motor filaments which he erroneously supposed were included with the sensitive filaments in the labial part of the infra-orbitary branch, having been cut across. But a different and more correct explanation of the occurrence (and I shall afterwards shew to whom we are indebted for it, and what degree of credit is due for making it) has since been given. It is now to be admitted that the seeming loss of power was not the effect of the motor nerves of the part having been destroyed or diminished in their influence; but that it was due altogether to the *sensation* of the lips being lost. It may be understood, that for the actions of a part like the lips to be perfect, it is not only necessary that the nerves of motion shall possess their power unimpaired, but that the nerves of sensation must likewise be entire: because it is only through a feeling

of the object held by the lips being conveyed to the sensorium, that the motion of the lips can be properly regulated. When the ass, after the division of the infraorbitary branch, which confers sensation, pressed its lips against the oats, it did not feel them, or was unconscious of their presence; and, consequently, it made no effort to gather them into its mouth. Hence all that the experiment, when correctly viewed, tended to shew, was merely that the labial, as well as the nasal and orbital, branches of the infraorbitary nerve, give sensation: and for combining the lips with the muscles of the jaws, in the actions of chewing the food, some other branch of the fifth pair must be sought for.*

Accordingly, it may now be perceived what was the effect of the failure of this experiment. It followed, that the only experimental proof which had been brought forward to illustrate the fact, that the fifth pair was not restricted to giving sensation exclusively, but, in virtue of its double roots, could bestow *motion* also, fell to the ground, and was rendered perfectly valueless. To one, therefore, who was ignorant of the principle of the subject, or depended for his opinions solely upon direct experiments, without regard to the anatomy of the roots, it is obvious that it must have appeared at that stage of the inquiries, that the fifth pair was incapable of

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* This office has been subsequently assigned by Sir Charles Bell to the *ramus labialis buccalis*, of the third division of the fifth.—*Nervous System*, p. 79.

giving motion, and was a mere sensitive nerve. But it is proper to mention here (even although it may be anticipating what I have hereafter to say), that as soon as the above error regarding the infraorbitary branch was exposed, Mr. John Shaw commenced a new series of experiments on the fifth pair, to remedy the defect. Leaving the superficial branches, which, by coming off from the ganglionic root alone, confer sensation simply, he chose the third division, in which the two roots (see the frontispiece) are joined together. He cut down upon this nerve* where it issues from the base of the skull, and just before it sends off its branches to the muscles of the jaws, in a living ass; and, having exposed it, he cut it across. The effect was clear and undoubted. As soon as the nerve was divided, the animal was deprived of the power of closing the jaws: and when the cut extremity was irritated by the forceps, the muscles acted convulsively, so as to bring the jaws together with force. Hence, although the first set of experiments was defective, inasmuch as they did not really prove that the fifth pair could bestow motor power, the second series completely established the accuracy of the original statement made by Sir Charles Bell—that the fifth corresponded with the spinal nerves, in bestowing motion and sensation, and was the "nerve of sensation and mastication," in the head.†

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* See "Medical and Physical Journal." October 1822.

† All the illustrations of the functions of the fifth pair, contained in Sir Charles Bell's first paper to the Royal Society, prove that he

I ought now, perhaps, before finishing this part of the subject, to draw some further attention to the

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was aware of this nerve being restricted, in its power of giving motion, to controlling the muscles of mastication. Without reverting to the instance in the text, where it is distinctly shewn that it was that portion of the infraorbitary branch alone which passes to the lips, a part of the organ of mastication, that was supposed to give motion, while the other portions of the same branch that go to the lower eyelid and nostril, which have nothing to do with that organ, were considered to be simply sensitive, I may present another example. Neither the eyebrow nor forehead can be said to form a part of the organ of mastication. It will, therefore, be found, that in experimenting on the supraorbitary branch of the fifth, which supplies these parts, Sir Charles Bell expressly stated that this branch had no power over the muscles. "I divided," he says, "the branch of the fifth pair, which goes to the forehead (supraorbitary), in a man, at his urgent request, on account of tic-douloureux: there followed no paralysis of the muscles of the eyebrow."—(P. 23.)

I may subjoin one or two additional illustrations.

"We have seen," Sir Charles remarks, "that when the fifth nerve—the nerve of mastication and sensation—was cut in an ass, the animal could no longer gather his food. In the individual whose face was paralysed on one side, during the excited state of respiration, there could be observed no debility or paralysis in the same muscles when *he took a morsel into his mouth and began to chew.* * * * In all animals that have a stomach with palpi, or tentacula, to embrace their food, the rudiments of this nerve (the fifth pair) may be perceived; and always in the vermes, that part of their nervous system is most easily discerned which surrounds the œsophagus near the mouth. If a feeler of any kind project from the head of an animal, be it the antenna of a lobster or the trunk of an elephant, it is a branch of this nerve which supplies sensibility to the member, and animates its muscles. But this is only if it be a simple organ of feeling, and is not, in its office, connected with respiration. From the nerve that comes off from the anterior ganglion of the leech,

analogy between the fifth cerebral nerve and the spinal nerves. But to prove the importance attached to this

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and which supplies its mouth, we may trace up through the gradations of animals a nerve of *taste and manducation*, until we arrive at the complete distributiou of the fifth, or trigeminus, in man. Here, in the highest link, as in the lowest, the nerve is subservient to the same functions. It is the nerve of taste and of the salivary glands; of the muscles of the face and jaws, and of common sensibility. * * * On cutting the respiratory nerve of the face (portio dura) in the carnivorous animals, it did not appear that the action of feeding was left so entire as in the graminivorous animals. This leads us to reflect on the different natures of the two classes. The beast of prey procures his food under the influence of a blood-thirsty appetite, and suffers a universal excitement; he holds and rends his prey; and, especially in the larger animals of this class, the action of feeding is accompanied with horrific sounds of enjoyment — in short, with a highly excited state of the organs of respiration: in the graminivorous animals, the act of feeding is a simple and unempassioned exercise of the organs of mastication. The author hopes that these experiments will be deemed conclusive; yet it is a pleasanter mode of investigation to have recourse to comparative anatomy. There is only one additional instance of this kind that he will offer. It has been already stated that when a feeler, or antenna, is examined, if it be simply for sensation, one nerve only runs along it. It was suggested to him that, if his theory were true, the trunk of the elephant being hollow, and connected with respiration, it should have two nerves; whereas, in the observations of Cuvier, it was stated to have only one. An opportunity of ascertaining the truth of this was very liberally granted by Mr. Herbert Mayo, who had lately a young elephant for dissection. The two nerves were readily found, both of great size; the one a continuation of the superior maxillary branch of the fifth, the other a continuation of the respiratory, or seventh." —(P. 24.)

I may be allowed to add, that in an original draught of the first paper to the Royal Society, which I have recently found among some cast-away MSS., the term *nervus devorans* is applied to the fifth pair.

observation, need not detain me long. It is sufficient to mention that these nerves, classed together by Sir Charles Bell on account of their resemblance both in their anatomy and their functions, formed the basis of his arrangement of the nervous system. The spinal nerves and fifth pair formed that class to which he applied the term "Original, or Regular System;" implying by that name a system found throughout the animal kingdom generally, and extensively distributed to all the moving and sensible parts of the body. The class with which they were contrasted, was termed the "Superadded System," and consisted of a comparatively small number of nerves, limited in their distribution, and only belonging to the higher classes of animals which respire by lungs. It is not my intention to dwell, in this part, upon the arrangement here referred to, for it will be better to reserve the discussion of that subject for the Conclusion; but I may be allowed to mention, that in none of the six different memoirs that were published before either Mr. Mayo or M. Magendie commenced their labours, was the account of this classification omitted. Of course, in these descriptions, the analogy between the fifth pair and spinal nerves was brought prominently forward. In the "Manual of Anatomy," published by Mr. John Shaw (September 1821), an engraving was given at the end of the volume, representing these nerves arranged together; and this engraving, it is stated, was taken from the class-drawing used by Sir Charles Bell in his lectures, while explaining

these subjects. The following is the description by Mr. Shaw, that accompanies the engraving:—

"Mr. Bell, in his late leetures on the nervous system, has shewn that all the spinal nerves, the suboeeipital, and the fifth, have several essential eireumstanees in eommon: —that they have eaeh two distinet roots—that they have eaeh a ganglion on one of their roots—that they are all exquisitely sensible—that they are all distributed to the museular frame, for loeomotion and aetion—that eaeh nerve is distributed to its corresponding division of the bodily frame, without ever taking a longitudinal eourse in the body—and, finally, that these nerves are eommon to all animals whieh have a symmetrieal body and a regular nervous system. This view will be more readily understood by referring to the plan in Plate I.

"When we examine the origins of the nerves minutely, we shall find that the fifth is the only nerve of the skull whieh comes off in sueh eritieal eireumstanees as to have a root from the erus eerebri and another from the crus eerebelli; whieh parts may, by eomparative anatomy, be proved to be the continuations of the anterior and posterior divisions of the spinal marrow.* The fifth will also be found to be the only nerve within the skull whieh has a ganglion at its roots. Those who have disseeted the deep nerves of the head, or who have attempted to demonstrate the branehes of the fifth pair to students, will be able to estimate the value of this view.

"I have examined the nerve repeatedly in its whole eourse, in man, in the horse, in the ass, the ealf, and the dog. By these disseetions I have been eonvineed that in

* See note, p. 63.

every respect the fifth pair resembles the spinal nerves, even in the peculiar form of its ganglion and plexus. In the horse there is as distinct a plexus formed by the branches of this nerve which go to the different parts of the head, as there is formed by those which go from the axilla, or loins, to supply the limbs. I conceive, also, that the form of the part from which this nerve arises, is analogous to that of the spinal marrow, where the axillary nerves take their origin. If this be correct, it will be another proof of the similarity of the fifth nerve to the spinal nerves.

"In this investigation I have also been able to correct the very common mistake, that the sympathetic nerve has its principal connexion with the nerves of the head through the sixth nerve. The branches of the sympathetic, which appear to go to the sixth, go to the ganglionic portion of the fifth. By the establishment of this fact it is proved, that even the connexion between the sympathetic and fifth is similar to the union of the sympathetic with the ganglionic roots of the spinal nerves.

"For an account of the experiments by which the similarity of the fifth and spinal nerves is further proved, I must refer to a paper in the 'Philosophical Transactions' of the present year."—(P. 256.)

The subjoined passages come under the head of "Explanation of the Plates:"—

"The principal arrangement is this:—there is an obvious division of the medulla spinalis, corresponding to the cerebrum and cerebellum; every REGULAR nerve has two roots, one from the anterior of these columns, and another from the posterior. Such are the fifth pair, the

suboccipital, the seven cervical, the twelve dorsal, the five lumbar, and the six sacral; viz. thirty-two perfect, regular, or double nerves. These are laid down in the first plan. They are common to all animals, from the worm up to man; and are for the purposes of common sensation and motion, or volition. They run out laterally to the regular divisions of the body, and never take a course longitudinal to the body.

"For the sake of arrangement (although the term be not correct, where every thing is perfect), the remaining nerves are called 'irregular' nerves. These are distinguished by their arising by a simple fasciculus, or single root; that is, a root from one column. * * * If we inquire into the reason of the seeming confusion in the second class, or "irregular" nerves, we shall perceive that it is owing to the complication of the superadded apparatus of respiration, and the variety of offices which this apparatus has to perform in the higher animals. To explain this, the second plan is given. It presents, in one view, the nerves destined to move the muscles in all the varieties of respiration, speech, and expression."—(P. 341.)

CHAPTER II.

REVIEW OF MR. MAYO'S FIRST MEMOIR, SHEWING HOW HE OPPOSED THE PRINCIPLE, THAT THE FUNCTIONS OF THE NERVES HAVE A RELATION TO THEIR ORIGIN AND STRUCTURE.

In the preceding chapter, I have endeavoured to present a simple view of how much had been accomplished towards establishing the fundamental principle of these researches, before any one else but Sir Charles Bell and Mr. John Shaw had commenced the prosecution of the same inquiries. I have accordingly carried the history down to that period when Mr. Mayo in this country, and M. Magendie abroad, began to take up the subject; and as it happened that both these gentlemen published their first memoirs in the same month (August 1822), it is of little consequence with which I commence my examination. I will therefore take Mr. Mayo's paper under review, in the first instance.*

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* The following is a list of the different publications treating of the nervous system that were given to the world by Sir Charles Bell and Mr. John Shaw, before August 1822:—

1. Idea of a New Anatomy of the Brain, submitted for the Observations of the Author's Friends; by Sir Charles Bell. 1811.

2. On the Arrangement of the Nerves; by the same. Phil. Trans. July 1821.

3. Manual of Anatomy; by Mr. John Shaw. 1st edition, September 1821; 2d and 3d editions, 1822.

4. On the Difference of the Functions of the Nerves of the Face; by the same. Quarterly Journal of Science, December 1821.

The first paper which this gentleman wrote was contained in a work treating of miscellaneous subjects, which he called his "Anatomical and Physiological Commentaries." It is also important to observe, that this work consisted of two distinct numbers, in each of which a paper on the nervous system was introduced. The first number, as has just been mentioned, appeared in August 1822; and the title of the paper it contained was, "*Experiments to determine the Influence of the Portio dura of the Seventh, and of the Facial Branches of the Fifth pair of Nerves.*" The second number was not published till nearly a year afterwards—namely, July 1823. It is to the former of these papers alone that I beg the reader's attention at present.

I would first remark, that, as Mr Mayo, before entering on these inquiries, had enjoyed the advantage of being Sir Charles Bell's pupil, and had been admitted, both by this gentleman and Mr Shaw, to pursue these researches along with them,* it was natural to expect that he would have appreciated the

5. On the Effects produced on the Human Countenance by Paralysis of the Different Systems of Facial Nerves; by Mr. John Shaw, in the Quarterly Journal of Science, March 1822.

6. On Partial Paralysis; by the same. Medico-Chirurgical Transactions, April 1822.

7. On the Nerves which associate the Muscles of the Chest; by Sir Charles Bell. Phil. Trans. May 1822.

* It is seen that Mr. Mayo's name occurs, in one of the quotations in the last chapter (p. 70, *note*), as having given Sir Charles Bell the opportunity of dissecting the nerves of the face in a young elephant. It is also twice mentioned, for a similar purpose, by Mr. John Shaw, namely, in his "Manual of Anatomy" (Sept. 1821), p. 259; and his paper, contained in the "Quarterly Journal of Science," (December 1821), p. 243.

novelty and importance of the new observations better than any other individual. But on examining his first paper we shall be disappointed. We shall find, from the whole tone and substance of his memoir, that he was not only ignorant of the most simple and essential circumstances connected with the discoveries, but that he occupied himself in endeavouring, as far as lay in his power, to controvert those facts which are now considered the most firmly established and the most important.

In the first place, Mr. Mayo displayed, even in the opening sentence of his paper, that he had no conception of the nature of the general principle on which these researches have been founded, and which it has been the grand object of every succeeding physiologist, who has joined in them with any degree of intelligence, to corroborate,—I refer to the law, that the nerves possess their distinct and appropriate endowments, in correspondence with their origins from the brain and spinal marrow. Instead of acknowledging the value of this principle, or adopting it as a guide, Mr Mayo commenced his memoir by directly opposing it, and endeavouring to re-establish the old and now abandoned system of pursuing the researches.

Since it is important for what is to follow, that I should place this point clearly before the reader, I may be pardoned for a little repetition. I would therefore remind him, that, according to the mode of prosecuting the inquiries that prevailed before Sir Charles Bell entered on the subject, it had been the universal custom to trust exclusively to experiments on living animals,

made without any previous consideration as to the distinct origins of the various nerves. Whether, for example, a nerve arose by one or by two roots; or whether one root had a ganglion upon it or not; or whether a nerve sprung from one column of the brain or spinal marrow, or another, was considered of no moment. Experiments were performed on the different nerves, in their course through the body, without attention to these important parts; and the results are known:—no progress was made towards discovering the distinctions in the functions of the nerves, or columns of the brain.

And here it deserves remark, that although it may not be difficult to understand how, from pursuing this course of investigation, physiologists should have failed to ascertain the cause why the *spinal* nerves bestowed both sensation and motion,—it is not so easy to comprehend how the same want of success should have attended the experiments upon the nerves of the face —the fifth pair and portio dura. In the former nerves —those of the spine—the root which confers motion, almost immediately after its departure from the spinal marrow, is joined by that which gives sensation; and the whole nerves are therefore compound, both in structure and function, nearly from their commencement: hence, unless the experimenter goes to the vertebral canal, and exposes the nerves in that situation, where the roots are separate, he will necessarily be deceived into the belief that it is the common property of these nerves to bestow motion and sensation conjointly. But the circumstances are altogether different,

with regard to the fifth pair and portio dura. If in any case, throughout the whole body, the old mode of pursuing the researches, by trusting to experiments alone, could be of any avail in elucidating the subject, it was to be expected that it would be manifested in treating of these nerves. And why? For this cause: the nerve which gives sensation to the surfaces of the face—the fifth pair—runs altogether apart from that which gives motion to the same parts—namely, the portio dura. Instead of the nerve of motion being united with the nerve of sensation, and forming a single compound trunk, as we find in the nerves of the body generally, the sensitive nerve issues from the skull, by distinct foramina, at a distance from the nerve of motion; and they remain separate until they coalesce at their extremities. Hence, when an experimenter exposed either of these nerves in one of the lower animals, and cut it across, it was natural to have expected that he would have observed, that motion exclusively was destroyed by dividing the portio dura; and sensation exclusively, by dividing the fifth pair.

What, however, does the history of these researches tell us? We know that men, the best qualified that the profession has ever seen, for prosecuting such inquiries, have absolutely failed to make these observations—even when the best opportunities occurred for making them. No better example of this fact can be adduced, than one brought forward by Sir Charles Bell in his work on the nervous system, and I may therefore briefly refer to it. I allude to a case related by the celebrated Dr. Darwin, in his "Zoonomia." A gentle-

man, it is stated, labouring under *tic-douloureux*, was attended by the famous Cruickshanks, and also by Mr. Thomas, now the respected President of the Royal College of Surgeons of London. The patient had all the different nerves of his face cut across in succession, for the relief of his complaint. Dr. Darwin mentions, that no less than nine incisions, "together with some smaller ones," were made by these two surgeons! We cannot, therefore, doubt that all the branches, both of the portio dura and of the fifth pair, were fully divided. The necessary consequence of these operations, we also know, would be, distortion of the countenance, and inability to close the eyelids, so that the eyes would remain permanently open, awake or asleep, on the one hand; and perfect loss of sensibility of the skin, on the other. Now, although we are certain that such results would follow, and that the patient would suffer the greatest distress from these effects of the operations, it is a remarkable fact that not a single observation was hazarded by Mr. Cruickshanks, Mr. Thomas, or Dr. Darwin, as to the functions of either of the nerves. The case terminates with the remark, that "the patient returned into Leicestershire perfectly restored."

It is, therefore, fair to infer, that when men so eminently qualified, as these gentlemen were, to take advantage of the old method of prosecuting the inquiries, as far as it was capable of being made a useful instrument of discovery—for the operations on this patient were experiments, made, too, under more favourable circumstances than when one of the lower animals was the subject—shewed that they were

unable to draw any conclusion with regard to the appropriate functions of these two distinct nerves, no reasonable hopes could be entertained of advancing the subject by continuing the same plan of investigation.*

But when Mr. Mayo entered on these inquiries, it was his first and chief object to try to subvert the principle recommended by Sir Charles Bell. Instead of admitting that any value deserved to be attached to the system of examining the roots of the nerves, previous to undertaking experiments, he commenced his paper by affirming indirectly that such a method was fallacious, and prejudicial in its influence :—

"The only unexceptionable evidence," Mr. Mayo remarks, "respecting the influence of individual nerves, consists in the record of cases in which, through accidental violence, or in surgical operations, single nerves have been divided in the living human body. In default of such evidence, the next measure is to collect the results of experiments made on animals. If by this method it be discovered that corresponding nerves, in different kinds of animals, have uniformly similar functions, it may be presumed that the like nerves have offices not materially different."

Having made these remarks, he proceeds to give a

* I subjoin a sentence from Sauvages, that shews how experiments, made without regard to the anatomy, are to be estimated :—"Scholastici fingunt ad convulsiones explicandas, alios nervos esse sensorios, alios vero esse tantum motorios, et sensus expertes : *quod cum millenis vivisectionibus falsum evincatur, admittendum non est:* nulla enim in corpore est fibra nervea quin sentiat."—*Nosol. Method.* tom. iii. p. 17. 1795.

description of the anatomy, first of the portio dura, and then of the fifth pair; and, in accordance with the plan he espouses, omits to take any notice of their distinct roots.

"With the view," he continues, "of contributing some materials to serve as data in an argument of this nature, I shall describe the distribution of the portio dura, and of the second and third divisions of the fifth in the ass, together with the phenomena ensuing on the division of several of their branches, and of that of the frontal nerve.

"The portio dura in the ass passes obliquely outwards and downwards after its exit from the cranium, being covered by the parotid gland, to which it adheres, and reaches the root of the condyloid process of the lower jaw."

And so he proceeds, describing all its ramifications in the same minute manner. Arriving next at the fifth pair, he enters on its description in this manner:—

"A frontal nerve, from the first division of the fifth, emerges upon the forehead.

"The second division of the fifth, after leaving the skull, crosses the spheno-maxillary fissure, in which it gives off four small branches, distributed to the posterior alveoli, the palate, and membrane of the nose."

The same minute description is continued till he comes to the third division; and although it is here that the ganglionic and non-ganglionic roots join, he still makes no reference to these parts.

"The third division," he observes, "is larger than the second. It sends off at once four branches: the first to the pterygoid muscles; a second, to the masseter and temporal muscles; a third, to join the portio dura, in the manner above described, a filament from which is reflected from the base of the skull, at the inside of the glenoid cavity; and another to the external ear."

And so he goes on. Can any one, therefore, question, that it was Mr. Mayo's express object, in this first paper, to overturn the new mode of investigating the functions of the nerves introduced by Sir Charles Bell? If there should be any doubt on the subject, it will be dispelled by attending to the circumstance which I have next to bring forward.

It has been seen, that in Mr. John Shaw's paper on "Partial Paralysis," a full description was presented of the experiments on the roots of the spinal nerves. This description was given principally for the object of shewing, that in order to ascertain the cause why these nerves should possess both motion and sensation, it was necessary to go to the roots, before coalescing, when performing experiments upon them. The account of these experiments, it is also to be noticed, was accompanied with observations as to the resemblance between the roots of the spinal nerves and of the fifth cerebral nerve, and the want of resemblance between them and the portio dura. It has also been shewn, that, as this paper was read to the Medico-Chirurgical Society in April 1822, and published in their "Transactions" in the June following, Mr. Mayo, who did not write his memoir till August, had ample time to become

familiarly acquainted with it before he composed his paper.*

Now it will be discovered, that, however great is the importance attached by the scientific world, at the present day, to the experiments on the roots of the spinal nerves,—Mr. Mayo, in his first paper, never once alluded to these experiments. He passed them over unnoticed—as of no value whatever.

Again: he took up the observations concerning the resemblance between the fifth pair and spinal nerves, a part of the discoveries the next greatly prized at the present time, and employed all his efforts to controvert that view.

First, with regard to his neglecting to draw attention to the experiments on the roots of the spinal nerves. We have abundant evidence that it could not have been from ignorance of these experiments having been previously made, that he passed them over; but that it must have been from another cause—inability to appreciate their value. Supposing Mr. Mayo had never heard of Mr. John Shaw's paper on "Partial Paralysis," I will presently shew that he had numerous opportunities besides, of learning the results of these experiments; and there is no other way of understanding how he should have omitted to take notice

* That Mr. Mayo had read, or heard read, Mr. Shaw's paper on "Partial Paralysis," can scarcely be doubted. At p. 119 of his memoir he introduces Mr. Shaw's name, as engaged with Sir Charles Bell in investigating cases of *partial paralysis*. "These inferences," he says, "are applied by Mr. Bell and Mr. Shaw to explain the phenomena of partial paralysis of the face."

of them, except by presuming that he could not see their importance.

It has been seen that, so far back as 1811, Sir Charles Bell gave a printed account of these experiments, in his "Idea of a New Anatomy of the Brain." Now, although I cannot positively say that Mr. Mayo received a copy of this unpublished essay, yet I may be allowed to assume that, as he was a house-pupil of the writer, it is probable that he was acquainted with the work.

Next; Sir Charles Bell, twice annually, delivered a course of lectures on anatomy and physiology. That in these courses he should have omitted taking notice of the experiments on the roots of the spinal nerves, when dwelling on the functions of the nervous system, cannot for a moment be credited. We have indirect evidence, indeed, that he not only described the results of these experiments, in his lectures; but that he pointed out, at the same time, the resemblance existing between the spinal nerves and the fifth pair. This we are entitled to infer, from knowing that the diagram engraved in Mr. John Shaw's "Manual of Anatomy," representing the fifth pair and spinal nerves together, was taken from a class drawing used by Sir Charles Bell at his lectures. Hence, as Mr. Mayo was a pupil of the school of Great Windmill Street, we cannot doubt that he must have heard the experiments in question frequently described, as well as known that the fifth pair was considered to resemble these nerves.

Independently of the opportunities mentioned

above, we shall find it difficult to resist the conclusion, that Mr. Mayo either assisted at the performance of the experiments on the roots of the spinal nerves, or, at least, witnessed them. It may be observed, by turning to the passage from Mr. John Shaw's paper on "Partial Paralysis" (p. 14), in which he treats of the experiments upon the spinal nerves, that he mentions, that since 1809, they had been "frequently repeated, and always with the same results." By turning to another paper by the same gentleman, in the "Medical and Physical Journal"* (a paper having no reference to Mr. Mayo's pursuits), it will be found that the date is given of one of the times in which these experiments were performed. It is stated that they were repeated in the month of March 1821; and it is added that they were performed publicly before the pupils attending the school where the researches were carried on. Now, I cannot state it as a fact, that Mr. Mayo was one of the pupils present on that occasion. But it deserves some attention, that a gentleman, who a short time afterwards joined Mr. Mayo in founding a new school of anatomy,—Mr. Cæsar Hawkins—was Mr. John Shaw's assistant in performing the experiments at that time; and supplied him, moreover, at their close, with notes of what they had jointly observed.†

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* October 1822.

† I subjoin the passages here referred to:—"As I have stated," Mr. John Shaw remarks, "the experiments alluded to above have been often repeated, and always with the same results. I shall here copy verbatim the notes which I drew up on the 21st of March, 1821, as

Accordingly, it is interesting to ascertain how Mr. Mayo was engaged, at the time, or shortly after the time, when these experiments performed by Mr. John Shaw, with the assistance of Mr. Hawkins, were exhibited to the pupils of Windmill Street School. This point I am enabled to determine pretty exactly. I have said, that in three different papers, published before Mr. Mayo contemplated joining in these inquiries, his

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memoranda of the results of some experiments * made with Mr. Bell upon that day, to be inserted in his private journal.

" But this part of the research (viz. certain experiments on the ninth and glosso-pharyngeal nerves) was hurried, in order to examine the spinal marrow. The bones of the spine were exposed at the lower part of the neck, by cutting through the muscles, &c. as quickly as possible: about two inches of the canal were exposed by sawing through the crura of the spinous processes. The proper sheath of the spinal marrow was not opened; but the whole being pulled to one side, the nerves were seen going off. On irritating the posterior origins, no convulsion was produced; neither was there any on pinching the ganglion: but on pinching the anterior roots, or on pinching the two sets of origins at the same moment, a convulsion took place on the corresponding muscles. This was repeated on several of the nerves, and in all with the same results. Several of the pupils who were assisting were satisfied of there being a marked difference between the two sets of fibres: indeed, it appeared almost quite satisfactory that there was no convulsion produced by irritating the posterior roots or the ganglion.

" Mr. Hawkins, who was my principal assistant, makes the following observations (and which, at the time, I copied into my notes): ' The spinal marrow was exposed immediately after death, while some of the muscles had still a slight convulsive motion. Upon irritating the poste-

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* " These experiments Mr. Bell was unwilling to repeat often on living animals, after having seen the effect once or twice on them; he contented himself with ascertaining the effect after the animal had been stunned."

name had been introduced, both by Sir Charles Bell and Mr. John Shaw, for having given them the opportunity of dissecting the fifth pair and portio dura in a young elephant. In one of these publications* the date is mentioned when this dissection took place. It was in the month of May 1821. Hence we perceive, that it was only five or six weeks after the experiments on the roots of the spinal nerves had been performed publicly by Mr. Shaw, that Mr. Mayo gave Sir Charles Bell and this gentleman an opportunity of making a dissection, directly connected with the investigations into the functions of the spinal nerves. Can we, therefore, suppose that either of them would have concealed from Mr. Mayo the results they had obtained so short a time before, in experimenting upon the roots of the spinal nerves?

Accordingly, I again put the question, whether it can be imagined that when Mr. Mayo enjoyed such

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rior root of the spinal nerves in three or four places, no effect was produced upon the neighbouring muscles; but when the anterior roots singly, or the whole spinal nerve, was pinched by the forceps or pricked by the scissors, an evident motion was produced in the muscles, not only perceptible to the eye, but when the third or fourth dorsal nerve was touched, the whole scapula moved in the hands of the assistant. This motion was not communicated to the muscles when the ganglion, which is formed on the posterior root within the sheath, was touched; neither did it follow an injury of the posterior column of the spinal marrow. The motion given to the muscles was not the slight tremulous motion arising from the natural irritability still remaining in them, but it was convulsive and spasmodic, and followed each successive prick of the scissors.'"—(P. 7.)

* Manual of Anatomy, by Mr. Shaw, p. 259.

numerous opportunities of being acquainted with the experiments on the roots of the spinal nerves, and yet, in his first paper, avoided taking any notice of them, it could have been for any other reason, but because he was incapable of appreciating the value of such experiments, or of such a mode of investigating the subject?

Secondly, I must draw attention to the reception which Mr. Mayo gave to the opinion advanced by Sir Charles Bell, that the fifth pair was the only cerebral nerve which, in the structure of its roots and its functions, bore a true resemblance to the spinal nerves. For this purpose I will subjoin an extract from his paper:—

"As nothing," Mr. Mayo remarks, "is so prejudicial to the interests of science as the temporary adoption of an unsound theory, I shall hazard a few remarks upon that of Mr. Bell:—

"Mr. Bell observes, that 'the nerves of the spine, the tenth or suboccipital, and the fifth, or the trigeminus of the system of Willis, constitute the original and symmetrical system,' which is equally found 'in the leech and worm;' that 'these nerves have all double origins; that they have all ganglia on one of their roots; that they go out laterally to certain divisions of the body; that they do not interfere to unite the divisions of the frame; that they are all muscular nerves, ordering the voluntary movements of the frame; that they are all exquisitely sensible, and the source of the common sensibility of the surface of the body; and that, when accurately represented on paper, they are seen to pervade every part.' On the other hand, Mr. Bell observes, that 'the par

vagum, the portio dura, the spinal accessory, the phrenic, and the posterior thoracic, are respiratory nerves;' that is to say, 'they connect the internal organs of respiration with the sensibilities of remote parts, and with the respiratory muscles, and are distinguished from the nerves of which we have been speaking, by many circumstances. They do not arise by double roots: they have no ganglions on their origins: they come off from the medulla oblongata, and upper part of the spinal marrow, and from this origin they diverge to these several remote parts of the frame which are combined in the act of respiration.'

"I shall endeavour to shew that the preceding distinction is not founded on correct observation, and that, in truth, the nerves which Mr. Bell terms 'respiratory' do not differ in any important respect, as a class, from those with which he contrasts them.

"1. The par vagum: this nerve has many roots, and has a ganglion near its origin. When the branches of the par vagum which pass to the larynx are divided, the voluntary movements of that organ are destroyed: the part is no longer competent to the formation of sounds, or to assist in the act of deglutition: while, on the other hand, respiration is not impeded. The par vagum is acutely sensible: I exposed its trunk in the neck of an ass, and on pinching it with the forceps the animal gave violent indications of pain.

"2. The portio dura of the seventh is proved, by the experiments which I have detailed, to be a common nerve of voluntary motion: if it be divided, the muscles which receive branches from it are completely paralysed."

Here, then, we have an opportunity of judging of the value attached by Mr. Mayo, to the observa-

tion of the identity between the fifth pair and spinal nerves. No greater reason, he affirms, can be given for classing the fifth, than the par vagum or portio dura, with the spinal nerves. Let me, therefore, examine in what manner he supports his criticism; and for this purpose I will take the portio dura first.

The portio dura, it cannot be disputed, arises from the brain by a single root alone. Mr. Mayo, however, takes no notice of this circumstance. Although it was upon the very grounds that this nerve has a single root, while the fifth pair and spinal nerves arise by two roots, that the former nerve was put into a distinct class from the latter, Mr. Mayo has thought it altogether a matter of indifference to mention in what manner the portio dura originates! Again: it is one of the principal characteristics of the fifth pair and spinal nerves, that, in virtue of their double roots, they possess compound functions—they bestow both motion and sensation. Now, what does Mr. Mayo say in regard to the portio dura? Does he maintain that it also can give both motion and sensation? No; the only function that he ascribes to it (and so far he is correct) is motor power. Hence, because this nerve can give motion to the muscles, it is equal in its endowments to a series of nerves that can give, not only motion, but sensation besides! Lastly, he affirms, in positive terms, that the portio dura is "a common nerve of voluntary motion;" wishing obviously to infer, that it is incapable of controlling involuntary actions. But in this account of the nerve he is absolutely and unquestionably wrong. The portio

dura can control involuntary as well as voluntary actions; and there is no foundation for his calling it a common voluntary nerve.

In the next place, with regard to the par vagum. In order to prove that this nerve arises like the spinal nerves, Mr. Mayo says that—"it has many roots, and a ganglion near its origin." But in making this observation, he must have intended to cast ridicule on the subject. The spinal nerves and fifth pair do not arise by "many roots"—they have only two roots: and as for the ganglion formed upon them, it is not "near their origin," but upon their posterior or sensitive roots alone, that it is found.

In relation to this point, I must not let the opportunity pass of shewing, that it could not have been out of ignorance of the contrast between the origin of the par vagum and that of the spinal nerves, that Mr. Mayo made the above criticism. Long before publishing on this subject, and when Sir Charles Bell, we may presume, was engaged in dissections to establish the common nature of the fifth pair and spinal nerves, Mr. Mayo had been employed by the latter to make a preparation of the par vagum and spinal nerves. This preparation was afterwards deposited in the Museum of Great Windmill Street; and its express object was to exhibit the difference between the structure of the roots of the par vagum and those of the spinal nerves! This fact we learn from the same paper in the "Medical and Physical Journal" for October 1822, by Mr. John Shaw, to which I have already had to refer:—

"It would, perhaps, be well," Mr. Shaw remarks, "to make a short summing-up of the discoveries referred to in this sketch; but I will defer this, in the hopes that you will, in a succeeding number, permit me to offer you a short view of the whole question. Indeed, such a statement is now in some degree necessary; as those who are unacquainted with the variety of subjects involved in the inquiry may, perhaps, be swayed by some attacks which have been lately made upon it, and particularly by one who was for some years under Mr. Bell's roof. Though I am personally interested in the question, in consequence of the part I have taken in the performance of the experiments by which the views deduced by Mr. Bell from comparative anatomy were substantiated, still it is with much unwillingness that I take notice of a late pamphlet. * * I shall not enter into a refutation of the inferences which the author of the pamphlet has drawn from the experiments; because to those who are acquainted with what has been done in Windmill Street, during the last three years, in the prosecution of the anatomy of the nervous system, it must at once be evident that the results of the experiments repeated by the author . . . . afford additional proofs of the correctness of those views, which were originally drawn from anatomy.

"To those who are anatomists, it is needless to point out the incorrectness of the critique upon the origin of the par vagum. It cannot have been written in ignorance of the great difference which there is in the origins of the spinal nerves and the par vagum, as there is at this moment in the Museum of Great Windmill Street a preparation, made under Mr. Bell's directions, by the author of the pamphlet, to shew the very distinction which he attempts to confound."

Here, then, the fact is distinctly stated, that before the important discovery of the resemblance between the fifth cerebral nerve and the spinal nerves had been published, Mr. Mayo had been employed, under Sir Charles Bell's directions, in dissecting the only other nerve of the brain that could be supposed to bear any likeness to the spinal nerves, for the purpose of proving that no such similarity really existed between them. Does not this circumstance clearly shew, that when Mr. Mayo was a pupil he must have been privately instructed to view the origins of the nerves with especial interest? And is it not next to impossible, that he should have been put upon the task of making such a preparation as the one referred to, without being informed as to the experiments performed on the roots of the spinal nerves?

But I must not leave untouched the experiments on the par vagum, by which Mr. Mayo endeavoured to establish more fully the parallel between this nerve and the spinal nerves. They will afford a favourable opportunity for judging of the powers evinced by him of pursuing *original* researches.

In the first place: to prove that the par vagum could bestow sensation, by one of its "many roots," what course did Mr. Mayo pursue? Did he follow that recommended by Sir Charles Bell and Mr. John Shaw? Those gentlemen, with the view of ascertaining how the spinal nerves possessed the power of bestowing sensation, performed their experiments, not upon the trunks, but on the roots, of these nerves, before they coalesced. Mr. Mayo, on the contrary,

in proceeding to investigate how the par vagum gave sensation, chose the trunk of the nerve for his experiments, in preference to the roots. He cut down, in an ass, upon the par vagum, as it lies in the neck; that is, in a part where not only are its "many roots" united together in the same sheath, but where it has received besides numerous communicating branches from various cerebral and spinal nerves. And what was the next step he took, after thus exposing the nerve, in this situation? He *pinched* it; the consequence of which was, that the ass exhibited certain signs of pain. But what did these signs of pain indicate? Take any part of the body, an artery or a vein, and pinch it with forceps in an open wound; and the animal will manifest pain. Why did not Mr. Mayo follow the example set him by Sir Charles Bell, when experimenting on the branches of the fifth pair? To prove that the fifth nerve bestows sensation, this gentleman was not satisfied with pinching the nerve at the bottom of the wound. He cut across its several branches; and having thus insulated the integuments, supplied by the nerve, from the brain, he pinched these parts, namely, the integuments (not the nerve itself), with the view of ascertaining whether they retained their sensibility or not. What Mr. Mayo ought to have done, was to have cut across the par vagum, and then observed whether the organs to which it is distributed were deprived of sensation or not. By this means he might have ascertained whether the nerve was a sensitive nerve or not; although it is to be doubted whether he could have ascertained, by

such a course of experiments, on which of its "many roots" the sensibility depended.

In the second place, let me point out how he proceeded to make good his assertion—that this nerve could bestow "voluntary motion" like a spinal nerve. I must premise, that it is no part of my argument to shew that the par vagum is incapable of conferring such a kind of motion. But as Mr. Mayo has thought it important to establish this point experimentally, it may be interesting to observe what kinds of proof were satisfactory to his mind.

The particular branches of the par vagum which he fixed upon were the laryngeal—those that supply the organ of voice; or, in the ass, the animal selected for the experiment, the organ of braying. He divided, as he informs us, all these laryngeal branches in succession, in the same animal; by which we must understand, that he cut through not only the two superior, but the two inferior branches, making four nerves altogether that were divided. After this severe operation—"the voluntary movements of the organ," he says, "were destroyed, and the part was no longer *competent to the formation of sounds*." Interpreting the above language into a simpler form of speech, Mr. Mayo means to affirm, that after he had divided the four laryngeal branches of the ass, the animal could no longer bray. But, without denying the credit due to the great ingenuity and perseverance implied in ascertaining this point, I may be permitted to ask, What arts and manœuvres, did the learned experimenter employ, to induce the subject of his experiment to

attempt the "voluntary movement on his organ," as he is pleased to term it, after his throat had been cut in so many places, and which produced a result so satisfactory to Mr. Mayo?*

I have now to request the reader to follow me in giving another, and the last illustration I need present, of Mr. Mayo's ignorance of the correct principle of these discoveries. It relates to the functions of the fifth pair.

It has formerly been shewn, that the object which Sir Charles Bell had in view, in his experiments upon the fifth nerve, was to prove that, in correspondence with its arising by two distinct roots, it possessed motion and sensation conjointly. It has also been explained, that, in his attempt to establish this point, he unfortunately selected a branch, namely, the infra-orbitary, which, instead of possessing both these functions, is limited to bestowing sensation alone. Accordingly, his experiments did not bear out his assertion: they only led it to be supposed, on the contrary, that the fifth nerve was restricted to giving sensation.

Now it was through the joint labours of M. Magendie and Mr. Mayo, that this mistake was discovered.

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* The honest Sancho had no false shame about avowing, that he could bray in concert with another ass, so as to be mistaken for an ass:—"Que diable, quand j'étais petit garçon, je tirais vanité de savoir braire; personne ne s'avisait de m'en railler: au contraire, le plus huppés de mon village portait envie à mon talent. Ma foi! je ne veux pas vous cacher, que tous ceux qui me connaissent s'accorde à convenir que lorsque je me mets à braire, on croirait entendre un âne!"

By repeating the experiments on the infraorbitary branch, they shewed that this particular branch was incapable of bestowing motor power. To which of these gentlemen, however, the credit of making this correction is most justly due, I will leave it to others to decide. Suffice it to say, that the result of their experiments was what I have described, namely, to demonstrate that the infraorbitary branch provides sensation alone, and gives no power of motion.*

But this was the whole extent to which either of these two gentlemen went, in their investigations. And that is a circumstance to which I beg particular attention. After invalidating, or entirely destroying, the only experimental proof which Sir Charles Bell had

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* The following sentence is taken from M. Magendie's paper. He is speaking of Sir Charles Bell's experiments on the facial nerves:—

" Le resultat que nous avons obtenu s'accorde parfaitement avec celui que nous venons de rapporter—*à l'exception toutefois de la section du sous-orbitaire sur la mastication, influence qui n'a pas été évidente pour moi.*"—*Journ. de Physiol. Expérim.* Octobre 1821.

It was eleven months afterwards that, without making any acknowledgment to M. Magendie, Mr. Mayo repeated the same statement.

" The infraorbital and inferior maxillary branches of the fifth," he says, " were divided on either side, where they emerge from their respective canals: the lips did not lose their tone or customary apposition to each other, and to the teeth; but their sensibility seemed destroyed: when oats were offered it, the animal pressed its lips against the vessel which contained the food, and finally raised the latter with its tongue and teeth. On pinching with the forceps the extremities nearest the lips of the divided nerves, no movement whatever of the lips ensued. * * * Some days afterwards, though the animal did not raise its food with its lips, the latter seemed to be moved during mastication by their own muscles."—*Anat. and Phys. Comment.*, p. 110. August 1822.

brought forward to confirm his opinion that this nerve, by means of its two roots, could bestow motion as well as sensation—they suddenly ceased their inquiries! They did not prosecute the subject a step further! They did not consider it worth their while to make a single new experiment upon any other branch of the fifth, so as to put the question to the test, whether it really could bestow motor power or not. Accordingly, as they abandoned the inquiry, just after shewing that the only branch said by Sir Charles Bell to possess motor power in conjunction with sensation, had no such motor power, and was confined to giving sensation exclusively, they left it to be supposed that the fifth pair, instead of being what Sir Charles Bell considered it, a compound nerve, was provided for bestowing sensation, and sensation alone. Hence, it may justly be said, that they aimed a fatal blow against the principle which it was the main object to establish; inasmuch as their experiments tended to shew, that even although the fifth pair arose by two distinct roots, it could only minister to one office, namely, sensation.

It was Mr. John Shaw, as I have already mentioned, who observed the position into which the inquiries were brought by these two gentlemen; and who rectified the original mistake. He instituted a new set of experiments upon the fifth pair, which, in the most unquestionable manner, confirmed the truth of the opinions first expressed by Sir Charles Bell, with regard to its general functions; and these experiments he related in the same communication to the "Medical and Physical Journal," to which I have formerly referred,

as having been published immediately after Mr. Mayo's first paper, namely, in October 1822. Instead of experimenting on a superficial branch of the nerve, like the infraorbitary, he went to the third division of the nerve, which alone receives the motor root, and distributes its branches to the muscles of the jaws.

"I cut the nerve," he remarks, "nearer the brain, and at a point previous to its having given off the branches to the muscles; then the jaw fell, and the muscles of that side were powerless. I varied the experiment by irritating the nerve where it lies in the spheno-palatine fissure, immediately after an animal was killed; the jaws came together with force, indeed, so as to nip my assistant's fingers severely. This last experiment may be compared to the very common one of galvanising the nerves which pass from the spinal marrow, to supply the muscles of the extremities."

Let me next inquire, Whether there are any grounds for supposing, that when Mr. Mayo observed the error committed by Sir Charles Bell, in ascribing motor power to the infraorbitary branch, he was aware of the cause why this particular branch did not possess that kind of power? Did he know that the reason why it was incapable of giving motion was, that it comes from one root of the nerve simply, namely, the ganglionic, or sensitive root, and has no connexion with the motor root? This point it is important to determine, inasmuch as the whole interest attached to the correction depends on the question, whether the experiments referred to tended, or not, to corroborate the principle, that the functions of the nerves correspond with their origins from the brain.

It does not, however, require much trouble to settle this question. It might be sufficient, perhaps, if, in reply to it, I merely reminded the reader that, in pursuing these researches, Mr. Mayo treated the principle of attending to the roots of the nerves as of no value whatever: that, for example, in describing the anatomy of this very nerve—the fifth, he had avoided saying any thing about its roots. But to prove past all doubt, that when he made the observation concerning the infraorbitary branch being confined to giving sensation, he had no idea as to whether it was derived from the ganglionic root simply, or was composed of the two roots conjointly, or what its structure might be, I may present the following extract from his paper. We have already seen that, moved by his opposition to these new researches, he expressed himself hostile to nearly all that either Sir Charles Bell or Mr. John Shaw had advanced concerning the nervous system. But it deserves especial notice, that he made an exception in favour of one particular statement; and that statement referred to the functions of the infraorbitary branch! At the conclusion of his paper, we find this passage, in which he declares that, of all Sir Charles Bell's inferences, that only was correct which related to the infraorbitary branch; but he adds, that in his views regarding that nerve, Sir Charles Bell was not original: he was indebted for them to the previous labours of a Dr. Blair!

"It remains," Mr. Mayo observes, "for the reader to decide whether Mr. Bell's experiments are satisfactory, and

bear out his inferences; whether the latter, coupled with my former observations on the five respiratory nerves of this author, leave his theory tenable; and perhaps, finally, to determine whether there exist in the whole of Mr. Bell's Essay, after the deduction of his controvertible statements, *more than one correct inference*. I here allude to Mr. Bell's experimental confirmation of an opinion which, at the beginning of the eighteenth century, occurred to Dr. Blair, on his minute examination of the proboscis of an elephant, viz. *that the infraorbital nerves are nerves of touch*."

Could any thing prove more incontestibly Mr. Mayo's thorough ignorance of the whole merits of these new observations?—It makes all further comments on his first paper perfectly useless.

CHAPTER III.

MR. MAYO'S SECOND MEMOIR ESSENTIALLY DISTINCT FROM HIS FIRST; INASMUCH AS HE ABANDONED HIS OPPOSITION TO THE PRINCIPLE OF THE DISCOVERIES, AND ADOPTED IT FOR HIS OWN USE WITHOUT ACKNOWLEDGMENT.

Having devoted the last chapter to describing the principal subjects embraced in Mr. Mayo's first paper on the nerves, I must now request the reader's attention to his second memoir, published in the succeeding number of the same work in which the first appeared; namely, the "Anatomical and Physiological Commentaries."

I would begin by remarking, that although both these papers professed to treat of the same subject, so that the one was represented as the continuation of the other, never did any two productions, emanating from the same writer, differ so essentially from each other.

The second paper, it has already been mentioned, was not published till the month of July 1823; that is, eleven months after the first. Now, during that interval, both Sir Charles Bell and Mr. John Shaw had put forth numerous memoirs in addition to those which they had previously published. The different papers which they had given to the world up to that

time, amounted to twelve,* and in every one of them the same object was kept in view — namely, the full developement of the principle expounded for the first time in the "Essay on the Brain," printed in 1811 — that it is in correspondence with the distinctions in their origins from the columns of the brain and spinal marrow, that the nerves possess their distinct and appropriate functions. It must also be remarked, that, independently of what had been done by those two gentlemen, other physiologists, at home and abroad, attracted by the success attending the new mode of investigation, had likewise written on the same subject. Accordingly, when Mr. Mayo, in July 1823, published his second memoir, he had not only the advantage of examining with renewed attention the different papers which had appeared antecedently to the one he first produced, but also a number of additional ones given out in the intervening period.

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* In addition to the seven papers published before Mr. Mayo composed his first memoir, and the titles of which have been given at p. 75. Sir Charles Bell and Mr. Shaw had presented, before July 1823, the following five :—

1. Observations on M. Magendie's Experiments; by Mr. John Shaw. Medical and Physical Journal. October 1822.

2. On the Nervous System; by the same, in the same. December 1822.

3. On the Motions of the Eye; by Sir Charles Bell. Philos. Trans. March 1823.

4. On the Nerves of the Orbit; by the same, in the same. June 1823.

5. On the Nervous System. Second Part. By Mr. John Shaw. Medical and Physical Journal. June 1823.

To whatever cause we may ascribe the change, Mr. Mayo's second paper differed, I repeat, in every essential feature, from his first; and to such a remarkable degree, that, had his name not been affixed to the title-pages of both, we should not have believed they were the compositions of the same individual.

The chief and fundamental ground of difference between them was, that whereas in his previous paper he had made it an express object to reject the principle, that for investigating the functions of the nerves it was necessary to pay attention to their distinct roots, in his second paper he abandoned his ground, and adopted this very principle as the guide of his researches. Now, what makes this change the more important (as regards the present inquiry) is, that Mr. Mayo considered himself at liberty to alter his opinions, in this manner, without expressing a word of acknowledgment to Sir Charles Bell. In the whole course of his memoir, consisting of twenty pages, and treating exclusively of the functions of the roots of the spinal nerves, of the roots of the fifth pair, and of the roots of the various other cerebral nerves, neither Sir Charles Bell's nor Mr. John Shaw's name is once introduced! On the contrary, the very observations which were originally made by these two gentlemen are appropriated by him, as the results of his own unassisted investigations; and the only competitor he recognises is — M. Magendie.

The following sentences will confirm what I have

here stated. It is unnecessary, before giving them, to remind the reader that, in the preceding memoir, Mr. Mayo, when describing the anatomy of the fifth pair, had refused to advert to its double roots; and that he had, besides, opposed the statements made by Sir Charles Bell and Mr. John Shaw—that the fifth was the only cerebral nerve which bore a resemblance to the spinal nerves.

"Now, it is *well known*," Mr. Mayo remarks, "that the fifth nerve, at its origin, consists of two portions—a larger part, which alone enters the Gasserian ganglion; and another smaller, rising from the annular protuberance before the former, which subsequently does not enter, but passes below the ganglion, to join itself with the third division of the fifth, and escape with it through the foramen ovale. *Towards the close of last summer*, I endeavoured to trace the final distribution of this small portion in the ass, and succeeded in making out that it furnishes those branches which are distributed exclusively to muscles: this dissection I have repeated four times; and, in an adjoining drawing, have represented the fact as existing in the ass. I have since ascertained, that in the human body precisely the same distribution exists. But the remaining branches of the fifth are proved to be nerves of sensation. Thus it appears that the fifth nerve consists of two portions, one of which has no ganglion, and is a nerve of voluntary motion, and probably of muscular sensation; and another, which passes through a ganglion, and furnishes branches which are exclusively nerves of the special senses. Soemmerring, in his very excellent 'Treatise on Anatomy,' which I believe is universally received as the best extant, compares the fifth pair of nerves with the spinal

nerves. By this analogy I was led to conjecture, that the double roots of the spinal nerves have functions corresponding with those of the fifth; and that the large posterior portion of each spinal nerve, with its ganglion, belongs to cutaneous sensation, and the anterior branch to voluntary motion. When I was engaged in experiments to ascertain the fact, M. Magendie's were published, which establish the justness of my conjecture."—(P. 8.)

Here, then, we have a statement expressed in very clear terms, incapable of being misunderstood. On the one hand, Mr. Mayo represents himself as the originator of the discovery, that the functions of the fifth pair correspond with the distribution of its roots. On the other, he presents us with an explanation of the particular course by which he was led, in pursuance of his own unaided investigations, to institute a series of *original* experiments on the roots of the spinal nerves. I need not say, therefore, that it is incumbent on me to scrutinise his statements with some care.

Nothing, it is often remarked, is so useful, in matters of disputed evidence, as a date. Now, with this kind of test for judging of the accuracy of his representation, Mr. Mayo has supplied us in the above extract. It was "toward the close of last summer," and before M. Magendie had published his paper on the roots of the spinal nerves, he says, that he made the observations on the fifth pair and spinal nerves, which he so particularly details.

Before proceeding further, I must be allowed to

remark how necessary it was for Mr. Mayo, writing when he did, in July 1823, in order to establish a claim for originality, to make some such statement as the above. Granting, for the sake of argument, that M. Magendie was the only individual who interfered with his claims; if, instead of alleging that his observations concerning the fifth pair and spinal nerves had preceded the publication of this gentleman's memoir, Mr. Mayo had admitted that they were made after that time, it is obvious no one would have listened a moment to his representation, that he was led to perform his experiments on the roots of the spinal nerves, by having previously ascertained the distinct functions of the two roots of the fifth pair. Every person, on the contrary, would have said, that it was from having been acquainted beforehand with the experiments on the roots of the spinal nerves, published by the French physiologist, that he had been led to repeat these experiments; and that it was after this, he extended his observations to the roots of the analogous nerve, the fifth: that, in short, he followed the same course again, which Sir Charles Bell had pursued many years before, who commenced his investigations with the spinal nerves, and was subsequently led to examine the roots of the fifth pair.

To return to the date, therefore, of his observations: Is it probable that they were completed, as he says, "towards the close of the previous summer?" In the first place, we know that July is the last of the summer months. Accordingly, by this representation, they must have been finished sometime during that month

of the preceding year, 1822, at the latest. In the second place, he says they were made before the publication of M. Magendie's memoir. Now, this memoir was published in August 1822. Hence we have another reason for concluding that he meant it to be inferred, that it was not later than July 1822, when his researches were completed.

But how could Mr. Mayo so far forget the real circumstances? His own first paper, he ought to have remembered, in which he treated of the very same nerves, was published in August 1822. That is to say, his first paper was published at a later date than that to which he refers as the period when his supposed original investigations, concerning the roots of the fifth pair and spinal nerves, were completed. And need I ask, Whether, in that paper, there were any traces of his having entertained the views which he appropriates as his discoveries? So far was this from being the case, that every line in that memoir was intended to subvert these very opinions! Although it had been shewn, both by Sir Charles Bell and Mr. John Shaw, that the fifth pair possessed motion and sensation—and was the "nerve of sensation and mastication"—in correspondence with its two roots, while the portio dura, in correspondence with its single root, only possessed motor power—Mr. Mayo, in describing the anatomy of these two nerves, never adverted to the roots of either! Does this agree with his stating, in his second memoir, that it was "well known" to anatomists, that the fifth pair originated by two roots? Again, although Mr. Mayo had numerous

successive opportunities of being acquainted with the experiments on the roots of the spinal nerves, performed in the school of Great Windmill Street, and probably witnessed them, at least must have known the high importance attached to them,—did he not—obviously to shew how little he valued them—pass them over in his first paper, without offering a remark upon them? How is this to be reconciled with his stating that, before the date of that paper, he had been led, by his own unassisted researches, to perform a new series of experiments on these very nerves? How is it to be reconciled with his bringing forward M. Magendie's name, as the only person that stood in the way of his claiming these experiments as his own? In short, in whatever aspect we look at Mr. Mayo's representation of the course of his inquiries, we see nothing but the most palpable contradictions; and it is clear to demonstration, that before he made his supposed original experiments on the roots of the spinal nerves, he had opportunities of being not only intimately acquainted with those first performed in this country, but with those afterwards brought forward as original by M. Magendie.

It may, perhaps, appear superfluous to add any thing to what I have just stated. Nevertheless, I will offer one or two examples to shew how systematically Mr. Mayo seems to have neglected, when he wrote his second paper, all that had been previously published on the nervous system, either by Sir Charles Bell or Mr. John Shaw.

It has been seen that, in explaining the course by

which he was led to perform his supposed original experiments on the roots of the spinal nerves, Mr. Mayo alleges, that the idea was suggested to his mind from having, in the first place, investigated with success the distinct functions of the two roots of the fifth pair, and then perceived the anatomical analogy between this nerve and the spinal nerves. Now, putting aside for a moment the previous opposition with which he met this analogy, when advocated by Sir Charles Bell and Mr. John Shaw (see p. 89), what are we to think of his adopting this mode of accounting for his performing the experiments in question, when, ten months antecedently, in a paper communicated to the "Medical and Physical Journal" (October 1822), it had been openly stated by Mr. John Shaw, that while Mr. Mayo was yet a pupil, he had been employed by Sir Charles Bell to put up a preparation for the museum of Great Windmill Street School? the object of which was to demonstrate that the fifth was the only cerebral nerve which resembled the spinal nerves; or, at least, the purpose of which was to prove that the par vagum, which has a structure at its root, somewhat like a ganglion, has no true anatomical resemblance to the spinal nerves. Does not the fact of Mr. Mayo's having been put upon that task prove, in the most convincing manner, that it was from the suggestions and instructions he received from his former preceptor in anatomy, that he was led to pursue the dissections, which he represents as original, of the two roots of the fifth pair?

Again, Mr. Mayo alleges that before he entered on

his experiments on the roots of the spinal nerves, he had established, satisfactorily, that the fifth pair combined the power of giving motion with that of giving sensation. Now for this statement there is not, in reality, any foundation. No such facts were ever demonstrated by Mr. Mayo. Let the reader examine both this gentleman's memoirs with the greatest care, and he will not discover a single experimental proof of the fifth pair being capable of bestowing *motor* power. On the contrary, all the experiments which he has chosen to bring forward, and they amount to about five or six, have a directly opposite tendency to what he asserts; inasmuch as they all tend, without exception, to make it appear that the fifth is a nerve of mere *sensation.* And the reason of this it is not difficult to explain. Owing to Mr. Mayo having pertinaciously avoided, as it seems, acknowledging a single fact demonstrated either by Sir Charles Bell or Mr. John Shaw, he has omitted to take any notice of the experiments performed by the latter gentleman on the third division of the fifth pair, and which established that this nerve could bestow motion. That he might have been acquainted with these experiments before he composed his second paper, and known their importance, no one can entertain a doubt: for, before that time, they had been published by Mr. John Shaw, in no less than two different memoirs.* He might have been aware, too, that these were the only experiments by

* Medical and Physical Journal, October 1822; the same, December 1822.

which it was established that the fifth pair could bestow motor power. Nevertheless, he refrained altogether from making any allusion to them, and did not repeat them himself. Hence when, by his own account, he was led, from discovering that the fifth pair possessed both motion and sensation, to institute a series of original experiments on the roots of the spinal nerves, and to demonstrate why they also had motion and sensation, he had not a single proof in either of his papers to shew that the fifth pair really possessed motor power, or was any thing but a mere nerve of sensation!

The reflection, therefore, arises, how much more logically Mr. Mayo proceeded, in his first paper, to establish the analogy between the par vagum and the spinal nerves, than he did, in his second paper, when attempting to establish the analogy between the fifth pair and the spinal nerves. In the latter, we have just seen that he omitted to bring forward any experimental proof of the fifth pair being able to bestow motor power, like the spinal nerves; referring us only to a series of experiments, which would make it appear that this nerve was confined to bestowing sensation, in place of giving both sensation and motion. On the contrary, in experimenting upon the par vagum, with the view of exhibiting its supposed analogy to the spinal nerves, he made no such oversight. He not only performed a set of experiments upon the par vagum, to satisfy himself that it gave sensation; but he cut the four laryngeal branches of

this nerve, in order to demonstrate that it could also give motion, like the spinal nerves.

The only other illustration I shall offer to shew the utter ignorance in which Mr. Mayo professed to be, in his second paper, as to what had been laid before the public by those with whom he had been previously connected; or to prove how resolved he was to treat of the subject as if they had never participated in the inquiries, relates still to the experiments on the roots of the spinal nerves.

It has already been mentioned, that the experiments on the spinal nerves, originally performed in 1811, had been frequently repeated afterwards by Sir Charles Bell and Mr. John Shaw. One of the occasions on which they were repeated, we have seen (p. 86), was in March 1821, when Mr. John Shaw performed them before the pupils of the school of Great Windmill Street. It has also been stated that this gentleman published an account of these particular experiments, in his paper communicated to the "Medical and Physical Journal" of October 1822; that is, ten months before Mr. Mayo wrote his second memoir.

But the fact to be especially noticed is this, that, in giving the description referred to, Mr. John Shaw mentioned the name of the gentleman who assisted him in the performance of the experiments. He also inserted into his paper a memorandum containing the remarks of his assistant, as to what had been observed whilst performing them. The gentleman to whom I allude was Mr. Cæsar Hawkins. Now, it will be found that

although Mr. Mayo, in his second paper, scrupulously avoided mentioning the names either of Sir Charles Bell or Mr. John Shaw as having any connexion with these researches, he brought forward that of Mr. Cæsar Hawkins, for the purpose of acknowledging the assistance he himself had received from that gentleman:—

"This remark," Mr. Mayo observes, "I made and verified some years ago. My friend, Mr. Cæsar Hawkins, to whose assistance I am much indebted in the performance of the preceding experiments, recently at my request repeated this observation, and communicated to me an additional fact."—(P. 6.)

What comment need I make on the above sentence? Would it be possible to shew more emphatically how resolved Mr. Mayo appeared to be, to affect a total indifference as to what had been accomplished, in this country, in regard to these important discoveries, except by himself?

CHAPTER IV.

MR. MAYO'S OUTLINES OF PHYSIOLOGY.—HE ADHERES TO THE CLAIMS FOR DISCOVERY ADVANCED IN HIS SECOND MEMOIR.—SUPPRESSES FACTS RELATING TO SIR CHARLES BELL'S OBSERVATIONS, AND IMPUTES UNFOUNDED ERRORS TO HIM.—CONTRAST BETWEEN MR. MAYO'S ACCOUNT OF SIR CHARLES BELL'S VIEWS, AS GIVEN IN HIS FIRST MEMOIR AND IN HIS "OUTLINES OF PHYSIOLOGY."

In the two preceding chapters, I have given an account of Mr. Mayo's first and second papers, published in the "Anatomical and Physiological Commentaries;" I will now examine his statements concerning these discoveries, contained in the "Outlines of Physiology," a work not published till many years after the last paper to which we have been attending.

It must be observed, that as between the two memoirs published in the successive numbers of the "Commentaries," a remarkable contrast was shewn to exist, so an important difference will be discovered between the last of these memoirs and the chapter relating to the Nervous System in the "Outlines of Physiology." In Mr. Mayo's second paper, it has been perceived that the attempt was altogether avoided of giving a history of the course in which these researches were pursued. Nothing was done to enable the reader to form any judgment as to the relative share of merit due to those who had

been engaged in them; the names both of Sir Charles Bell and Mr. John Shaw were suppressed, as if they had never participated in the inquiries; and Mr. Mayo treated of all the most important parts of the discoveries as undoubtedly his own just property. But in the "Outlines of Physiology," it seems, he became sensible that this plan of proceeding was not satisfactory. Hence in this work he introduced, for the first time, what professed to be an historical account of the discoveries; so that, whereas in the second number of the "Commentaries," published close upon the time when the discoveries in question were first promulgated, and when all the circumstances connected with them were fresh in the minds of the public, he avoided explaining how much had been accomplished by one individual or by another—in his "Outlines of Physiology," which did not appear till all the particular facts relating to these questions were obliterated from the recollections of the public, he consented to bring his history forward.*

Another remark has to be made. It will be observed that Mr. Mayo, in giving this comparative sketch, in his "Outlines," has resorted to a remarkable plan for securing an advantage to himself over those whom he treats as his competitors. It has been shewn that it was in two distinct papers, pub-

* It has to be noticed, that Mr. Mayo allowed the first edition of his work to be published without the history referred to. It is only in the second edition, which appeared in 1829, that is, eight years after Sir Charles Bell's first paper to the Royal Socioty, that the history is to be found.

lished at an interval of eleven months, that he brought out the results of his labours. It has also been pointed out how completely the one of these papers differed, in the nature of its contents, from the other: that as, in the first, Mr. Mayo shewed unquestionably that he misapprehended the fundamental principle which formed the basis of all these discoveries, and, from this cause, fell into gross errors, endeavouring to controvert what he was afterwards obliged to acknowledge as correct, and omitting to notice things of the highest importance which he subsequently brought forward; so, in the second, he repaired all these mistakes and omissions by becoming acquainted, at last, with the principle of investigation alluded to, and pursuing the inquiries in the correct mode. Now, when referring, in his "Outlines of Physiology" to these early works, his "Anatomical and Physiological Commentaries," Mr. Mayo omits to explain that they consisted of two distinct numbers. He represents them, on the contrary, as a single production, published at one time; and in giving the date, to mark when this supposed single work appeared, he takes that of the first, instead of the second paper. On the only occasion, I repeat, in which he refers to his "Commentaries," and introduces a date, he gives it as 1822 (not, let it be remarked, August 1822, which would have been correct so far, but with the year simply in which the first number was published*), instead of July 1823. And

* Outlines of Physiology, 1837, p. 174.

in correspondence with this confounding of the dates of his two memoirs, he confounds also their contents; that is to say, in relating what were the facts and opinions brought forward by him in these early works, he makes no distinction between what was contained in his first and in his second memoir; but, on the contrary, gives it to be understood, in the most unequivocal manner, that they were all deduced from the same principle, and published at the same time. Accordingly, he assumes to himself the credit of having ascertained in 1822 (and, for aught he says, it might have been in the very beginning of that year) most important facts connected with these discoveries, which were never even alluded to by him till so late as July 1823.

But the reader would deceive himself if he thought that Mr. Mayo granted a similar advantage to Sir Charles Bell.

In the first place, he suppresses Mr. John Shaw's name — avoids ever referring to that gentleman's numerous publications in explanation of Sir Charles Bell's views; and in that manner escapes introducing him as a competitor.

In the next place, in giving a description of what was accomplished by Sir Charles Bell before he began his own labours, he confines himself to the examination of two of that gentleman's works alone. These are the unpublished "Essay on the Brain," printed in 1811, on the one hand; and the introductory paper on the arrangement of the nerves, presented to the Royal Society in July 1821, on the other. Accord-

ingly, although he himself takes the advantage of using the contents of a paper published in July 1823, for representing his own share in these discoveries, he will not give Sir Charles Bell the benefit of any work later than July 1821. Hence, when there were, as I have shewn (p. 104), no fewer than twelve distinct publications of different kinds, the joint productions of Sir Charles Bell and Mr. John Shaw, each of which he ought to have examined, before mentioning the contents of his paper of July 1823, he has only referred to two of them.

In order to be satisfied of the correctness of what is here stated, and to shew the necessity of distinguishing Mr. Mayo's two memoirs from each other, I beg the reader to turn his eye back to the extract from the "Outlines of Physiology," introduced at page 19. He will there perceive, that the subjects claimed as his particular discoveries, by Mr. Mayo, are those alone that he brought forward, for the first time, in the second number of his "Commentaries," and had omitted to notice in his previous number. That is to say, in his "Outlines of Physiology," he repeats the same statement which he had made in his second memoir—that it was from having examined the distinctions between the roots of the different nerves of the brain, that he was led to institute his experiments; and particularly, that it was from perceiving that the portio dura, which arises by a single, ganglionless root, has only one function; while the fifth pair, which arises both by a ganglionic and non-ganglionic root, has two distinct endowments, that he was

induced to commence his experiments on the double roots of the spinal nerves: which experiments, he adds, were completed before the publication of those performed by M. Magendie.

It now becomes my unpleasant task to explain the manner in which he endeavours to support these claims. When advancing them formerly, it has been seen, that he freed himself from the necessity of contrasting them with those of Sir Charles Bell and Mr. John Shaw, by a simple process—that of suppressing the names of both these gentlemen, and appropriating whatever he chose to adopt without any discussion. But in the present work, as I have already stated, he only suppresses Mr. John Shaw's name. It is, therefore, incumbent on me to examine his statements with more than common care, since we find him thus compelled to change his plan, and explain openly what one, at least, of these gentlemen had ascertained, before he himself had commenced his researches.

The course Mr. Mayo pursues is this. On the one hand, taking the nerves above referred to, he appropriates to himself the most valued parts of the observations connected with them; and this he does without mentioning whether the same views had been explained beforehand, or not, by Sir Charles Bell; thereby leaving it to be inferred that this gentleman had no claim for priority with regard to them. On the other, he imputes to Sir Charles Bell a series of errors concerning the same nerves, without adducing

any proofs, that deserve the name, in support of his statements.

It will be my duty to shew, that in whatever light we regard his representation, it is, in reality, without foundation. As to the errors, for example, which he has imputed to Sir Charles Bell, I will prove incontestibly that that gentleman never committed them; and that, consequently, they never could be rectified by Mr. Mayo. Neither in Sir Charles Bell's works, nor those of Mr. John Shaw, nor those of any contemporary writer who may have taken notice of their views when first promulgated, will any indication whatever be found of the truth of Mr. Mayo's statements. And, what is more, in his own "Commentaries," the work to which he so confidently refers, as containing the exposure and correction of the supposed errors, there is not a single allusion to Sir Charles Bell having held the particular views which, in his "Outlines of Physiology," he has ascribed to him. I will take the first number of the "Anatomical Commentaries," the only number, be it observed, in which Mr. Mayo reviewed this gentleman's observations or mentioned his name; and one, be it also remembered, that he published before he contemplated putting forward his claims to the discovery of the distinct functions of the roots of the fifth pair and spinal nerves (for it was in the second number alone that these claims were advanced); and I will shew that, in reporting what were Sir Charles Bell's views in that first paper, he never gave a single

hint, nor made a distant allusion, to his having committed the errors referred to, far less did he himself endeavour to rectify them. In short, however calculated Mr. Mayo's representation of Sir Charles Bell's original opinions, as given in the "Outlines of Physiology," may be, to clothe his own pretensions as a discoverer with plausibility, I will prove that it is utterly void of foundation, and was never brought forward till after the publication of the second number of his "Commentaries," in which he first set up his claims as the discoverer of the facts of which he treats.

What, then, are the particular points that Mr. Mayo has appropriated to himself as his own discoveries, without regard to whether they had been previously announced by Sir Charles Bell or not? and what are the errors that he has imputed to that gentleman?

Amongst the former, none is more remarkable than his venturing to suppress the fact, that it is to Sir Charles Bell we are indebted for introducing the new mode of investigating the functions of the nerves, by examining the distinctions between their origins; and then, indirectly, claiming that discovery for himself.

If we were called upon to state which, among all the different results of the new mode of investigation, were the most important, we should be obliged to point, in the first place, to the observation, that the functions possessed by the spinal nerves, by the fifth pair, and the portio dura, all correspond with their roots — that the spinal nerves give motion by one of their roots, and sensation

by the other — that the fifth pair is likewise motor by one of its roots, and sensitive by its other — and that the portio dura is confined to giving motion alone, on account of its having only a single root; and we should have to include, in the next place, the discovery that the fifth is the only cerebral nerve which resembles those of the spine. Now, these are the very points, as we have already seen, which Mr. Mayo assumes as his own discoveries! And if the reader will turn to the "Outlines of Physiology," he will perceive that he has appropriated them, without acknowledging that any other individual but himself had ever thought of prosecuting the researches, by examining the distinctions between the roots. In describing Sir Charles Bell's views concerning the spinal nerves, he has allowed, it is true, that this gentleman had performed certain experiments on their roots; but he represents him as having had, when instituting them, a totally different object in view from that of proving that the one root was provided for one function, and the other for a different function: on the contrary, he represents him as assigning two distinct functions to each of the roots. Then, with regard to the fifth pair and portio dura, Mr. Mayo nowhere mentions that Sir Charles Bell had described the difference between the roots of these two nerves: he does not even allow that this gentleman was aware of the fact, that the one arises by a double and the other by a single root. Lastly, as to the analogy between the fifth pair and the spinal nerves, he has kept out of sight that this had been ever dwelt upon by

Sir Charles Bell. How, then, can we interpret this proceeding in any other way, than as virtually assuming the merit of first suggesting and establishing the principle—that the functions of the nerves correspond with their roots—than as appropriating indirectly the fundamental part of the discoveries to himself?

But what are we to think of Mr. Mayo, in putting forward these important claims, referring us, for the confirmation of them, to the first number of his "Commentaries?" In that first number, it was his avowed and unequivocal object to contend against the very principle referred to! Instead of having been unwilling, when writing that paper, to let it be known that Sir Charles Bell employed the examination of the distinct roots of the nerves as his guide in investigating their functions, he commenced his paper by opposing, in the very first sentence, that mode of research; and maintained that to experiment on the trunks of the nerves, without reference to their roots, was the only unquestionable way of attaining truth. Thus, so far was he from acknowledging that, in regard to the spinal nerves, any advantage was to be gained by experimenting on their roots, and so little had he the idea, at that time, of claiming originality for performing such experiments, that he absolutely refused to take any notice of the experiments which had been repeatedly performed in this country on these nerves; and with which, from publications, lectures, and his connexion with Sir Charles Bell and Mr. John Shaw, and with the pupils of Great Windmill Street, he had the most ample means of being intimately acquainted! Again, in place

of considering it of any use, in pursuing the functions of the portio dura and fifth pair, to inquire into the difference between their roots, or being led to perform experiments on the roots of the spinal nerves in consequence of his investigations into their roots—it has been already shewn (p. 82) that Mr. Mayo gave the strongest possible evidence of his entertaining a totally different opinion, by undertaking a long anatomical description of these two nerves, without introducing a single remark about their roots—without so much as mentioning that the portio dura arises by a single, and the fifth pair by a double root! Lastly, instead of avoiding to mention, in his first paper, that Sir Charles Bell had established the analogy between the fifth pair and spinal nerves, Mr. Mayo, eagerly seized upon this part of his observations, as deserving his especial opposition! He mustered together a set of arguments—if arguments they can be called—to controvert it. Regarding truly the classification of the fifth pair with the spinal nerves as the most important part of the arrangement of the nervous system proposed by Sir Charles Bell, he began describing this gentleman's views concerning these nerves, by remarking that, "for the interests of science, nothing was so prejudicial as the adoption of an unsound theory." Did that indicate his acquiescence in the analogy? Then he followed up this sentiment by attempting to convince his readers, that both the portio dura and the par vagum bore as striking a resemblance to the spinal nerves, as the fifth pair. With regard to the par vagum, he not only instituted a series of experiments

upon it, for the purpose of proving that, like the spinal nerves, it ministered to motion and sensation; but he maintained that, inasmuch as this nerve "had many roots, and a ganglion near its origin," the identity was complete between it and the spinal nerves and fifth pair; although, at the same time, we know, on the authority of Mr. John Shaw, that when the investigations were in progress for establishing that the fifth was the only cerebral nerve that resembled the spinal nerves, Mr. Mayo had been employed by Sir Charles Bell to make a preparation, to shew that the root of the par vagum was altogether unlike those of the spinal nerves or fifth pair.

I must next pass to the examination of those direct errors that Mr. Mayo has imputed to Sir Charles Bell. These, it will be found, are of such a nature as to give additional strength to the impression that, when investigating the nerves, Sir Charles never attached any importance to their roots; and that it was reserved for Mr. Mayo himself to lay the foundation of that principle.

I will take, in the first place, what Mr. Mayo says of the fifth pair—the nerve that deserves our principal attention, from its having been in consequence, as he represents, of ascertaining the distinct functions of the roots of this nerve, that he was led to institute his alleged original experiments on the spinal nerves. It is obvious that, if, in describing Sir Charles Bell's views concerning the fifth nerve, he had allowed that that gentleman ascribed functions to it which denoted his acquaintance with the distinctions between its roots

— it would have been fatal, notwithstanding his omitting to mention the description of the roots given by Sir Charles, to his own claims as a discoverer.

It is now, perhaps, fully understood, that the fifth pair is peculiar—especially as contrasted with the spinal nerves—in arising by roots of unequal dimensions. The root which confers motion is only about a quarter the size of that which gives sensation. Hence it follows that this nerve cannot give motion and sensation in all its branches equally: only a limited part of the nerve can be endowed with motion, while the greater part will possess sensation. Whoever, therefore, should assert, in speaking of the functions of the fifth pair, that it could give motion and sensation conjointly in *all* its branches, indiscriminately and without distinction, would betray complete ignorance as to the structure and distribution of the roots of which it consists, and of the fundamental principle of the researches. On the contrary, if another person shewed that he was acquainted with the fact that the head universally received sensation from this nerve, but that it was the muscles of the jaws alone that obtained motion from it—we should then be compelled to conclude that he knew perfectly well both the anatomical structure of its roots, and that the one root (the lesser) was provided for motion, while the other (the larger) ministered to sensation. Hence, if, even without saying anything about its origin, he had designated the fifth pair as "the nerve of sensation and mastication," we should have been assured, by his adopting this name, that he had been guided in his

inquiries by the knowledge that a nerve could only give motion and sensation by its having two roots, one appropriated for motion, and the other for sensation; and that the fifth pair was a nerve of that character.

What opinions, then, has Mr. Mayo ascribed to Sir Charles Bell, concerning this nerve? He has represented him as stating that the fifth pair could give motion and sensation in every one of its branches, without distinction! His expression is, that Sir Charles Bell considered this an "ordinary nerve:" meaning to imply by that term, that, as it was formerly conceived that every nerve, whatever might be its origin, had the twofold capacity of giving motion and sensation, so Sir Charles supposed that the fifth was a nerve of that description. Accordingly, by ascribing such a view of the fifth to this gentleman, and omitting to say anything about the importance he attached to the examination of the roots of the nerves, Mr. Mayo has taken the very course to make it be believed, that Sir Charles Bell must have been ignorant of the distinctions between the roots of the fifth.

The above representation concerning the fifth pair, it may be perceived, could not be considered complete, unless some explanation were also offered of the supposed opinions held by Sir Charles Bell regarding the portio dura — the nerve which is associated with the fifth in supplying the face. From making it appear that the facial branches of the fifth, which are confined to giving sensation, were considered by this gentleman as conferring motion likewise; and from these nerves supplying parts already provided

with a motor nerve, in the portio dura; Mr. Mayo apparently saw the necessity of giving a representation of Sir Charles Bell's views concerning the portio dura, that might harmonize with his statement about the fifth, and account in some measure for this gentleman supposing the same parts to have two nerves of motion. Accordingly he has affirmed, that the kind of motion which Sir Charles Bell ascribed to the fifth pair was *voluntary* motion: while that which he ascribed to the portio dura was *involuntary*, or *instinctive*.

The following extracts from the "Outlines of Physiology" will confirm what has been stated:—

"Sir Charles Bell attempted to prove that there are different nerves for the transmission of the instinctive and of the voluntary impulse to muscles. But the experimental evidence which he advanced in support of his theory was fallacious. His experiments were made upon the branches of the fifth and seventh nerves, which supply the integuments of the nostrils and of the lips in the ass. They appear to have been suggested by the following views. The muscles of the face are remarkable for exhibiting at one time instinctive action, as in the play of the features in expression; while at another they are certainly moved by a deliberate effort of the will, as when food is seized by the lips. But the muscles of the face have been thought to be further distinguished from the muscles of most other parts, by having two nerves distributed to them; one a portion of the fifth, the other a portion of the seventh nerve. Is it not likely, then, reasoned Sir Charles, that one of these nerves (and he chose the seventh) is a nerve superadded to minister to

that instinctive action so conspicuous in the facial muscles; while the other (the fifth) is the nerve of ordinary qualities, intended to minister in the same parts to sensation and voluntary motion?"—(P. 173.)

"And as he saw that, after the division of one seventh nerve (portio dura), the nostril on that side ceased to move in breathing (an instinctive action), although the lips continued to be employed in seizing food (a deliberate and voluntary action), Sir Charles Bell concluded that he had obtained, by this experiment, a proof *that the seventh nerve controls the instinctive actions, but not the voluntary actions, of the muscles of the face.*"*—(P. 174.)

"Branches of the fifth pair, which Sir Charles Bell had supposed to be nerves both of sensation and voluntary motion."—*Ibid.*

"In other words, according to Sir Charles Bell, the seventh is the nerve of instinctive motion to the face, and the fifth of voluntary motion and sensation."—(P. 254.)

"The inference which Sir Charles Bell drew from these experiments was that the branches of the fifth, which emerge upon the face, to supply the muscles and integuments, are for sensation and voluntary motion jointly."—*Ibid.*

"If the facial branches of the fifth be not, as Sir Charles Bell supposed, nerves of motion as well as of sensation, &c."—(P. 255.)

From the reiteration of the same statement in so many separate passages, it is easy to perceive how earnestly Mr. Mayo wished it to be believed, that

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* The words in Italics, from being so printed in the original, might be supposed to be those of Sir Charles Bell: but they are Mr. Mayo's.

Sir Charles Bell supposed that the fifth pair bestowed *motion* as well as sensation, in *all* its branches. But what proofs has he brought forward in vindication of his assertions?

If the reader will refer to Mr. Mayo's work, he will find that only one example is presented, giving the least colour to his representation. And that one relates to the error committed by Sir Charles Bell with regard to the infraorbitary branch. Because, from knowing that this branch supplies a part of the organ of mastication, Sir Charles had erroneously conceived that the labial filaments (and these alone) gave both motion and sensation, Mr. Mayo has thought himself justified in alleging that he supposed every other branch of the fifth nerve to be capable of bestowing these two properties! Such is the kind of demonstration with which he seeks to satisfy his readers.

But let me inquire how far this representation about the infraorbitary branch agrees with what is stated concerning Sir Charles Bell's original views, in the first number of the "Anatomical Commentaries," published before Mr. Mayo had any intention of shewing, either directly or indirectly, that Sir Charles was ignorant of the distinctions between the roots of the fifth.

First. What are we to understand by Mr. Mayo's bringing this error against Sir Charles Bell, in his "Outlines of Physiology," when, in his "Commentaries," he had actually gone out of his way to allege that there was no error connected with the infra-

orbitary branch? The reader need not be reminded (see p. 101) that at the conclusion of his paper, when summing up his observations on Sir Charles Bell and Mr. John Shaw's researches, Mr. Mayo reverted to the statement concerning the infraorbitary branch, and observed that it was unexceptionable,—perfectly accurate! But he was not even content with this. He added that, taking into view all the assumed discoveries of these two gentlemen, the account of the functions of the infraorbitary branch, was the "only correct part" to which he could attach any credit. And in bestowing this praise on the statement, what did he represent were Sir Charles Bell's opinions? Did he display any desire to prove that this gentleman had ascribed both motion and sensation to the infraorbitary nerve?—Quite the reverse. He stated what was not correct—that Sir Charles Bell had regarded the infraorbitary branch as confined to giving sensation! and, on the strength of this representation, attempted to make it appear that an injustice had been committed towards a Dr. Blair; who, it seems, from dissecting the nerves of the proboscis in an elephant, had expressed the same opinion some time before, namely, that the infraorbitary was a nerve of *touch*. Thus, this single experiment is brought forward by Mr. Mayo, at one time, to prove that Sir Charles Bell was right, and at another time, to shew that he was wrong!

Secondly. Did Mr. Mayo, when treating of Sir Charles Bell's views of the fifth, in the first number of his "Commentaries," confine himself, as he has

done in the "Outlines of Physiology," to mentioning that single experiment on the infraorbitary branch? No; such was not the case; and I beg the reader's especial attention to the circumstance. Although, in so many distinct passages of his "Outlines," he has asserted that *every* facial branch of the fifth was supposed by Sir Charles Bell to combine the power of giving motion with that of sensation, he has omitted to bring forward an experiment made by that gentleman upon a particular branch, which, if he had produced it, would have at once overthrown his statement. Yet that same experiment he had introduced in the first number of his "Commentaries!" I refer to the operation, or experiment, of cutting through the supraorbitary or frontal branch of the fifth pair, in a man, for tic-douloureux. This operation was adduced by Sir Charles Bell to shew that, as the parts supplied by the frontal branch are altogether removed from the organ of mastication, and as the fifth pair could only control the muscles engaged in that office, this frontal branch had no influence as a motor nerve, and ministered to sensation exclusively. I transcribe the sentence relating to the operation from Mr. Mayo's "Commentaries."

"On the division," he says, "of the branch of the fifth pair which goes to the forehead on account of tic-douloureux, no paralysis of the muscles of the eyebrow followed." —(P. 118.)

Will Mr. Mayo, therefore, explain why he has omitted, in the "Outlines of Physiology," such a

material observation — one which, in the first number of his "Commentaries," he had no reluctance to introduce?

Lastly. It has been frequently stated that, to denote the general functions of the fifth pair, Sir Charles Bell designated it the "nerve of sensation and mastication." It has also been seen, that the application of this name affords undoubted evidence that he was fully acquainted with the distinctions between its roots. Mr. Mayo, however, in treating of this gentleman's supposed original views, in his "Outlines of Physiology," has omitted to acknowledge that Sir Charles gave a name to the fifth so justly descriptive of its functions; and asserts that he called it an "ordinary nerve;" which term, according to the interpretation that accompanies it, would lead us to infer that he had never reflected upon its roots. Now, what was the name that, in the first number of the "Commentaries," Mr. Mayo said that Sir Charles Bell had given to the fifth? Did he then exhibit any unwillingness to allow that Sir Charles had called it the "nerve of sensation and mastication?" Let the reader judge by the following sentence, extracted from that number of the "Commentaries:"—

"Mr. Bell derives the following inferenees, — that the nerves of the fifth pair are the 'original and symmetrieal nerves' of the face—imparting sensibility to it, and *exciting its muscles to the prehension of food.*"—(P. 118.)

Why has Mr. Mayo, in his "Outlines of Physiology," not adhered to the same term that he originally

used in his "Commentaries?" What can have induced him to make so important a change?

Let me now leave the fifth pair and examine his statements concerning the portio dura. What foundation has he for alleging, that the portio dura was considered by Sir Charles Bell to be an "instinctive nerve;" or, to repeat Mr. Mayo's expression, a nerve which "ministered to that instinctive action so conspicuous in the facial muscles?"

The first observation I would offer is this:—although, in the "Outlines of Physiology," the term "instinctive," as applied to denote Sir Charles Bell's supposed opinions concerning the portio dura, is used in no less than six different passages; and the major part of a chapter, headed upon instinct, is devoted to controverting his alleged views; the word "instinctive" itself will not be found in any part of this gentleman's writings; or, I may add, in any of Mr. John Shaw's writings; or in those of any other contemporary author, who professed to give an account of Sir Charles Bell's opinions! What is more than this—in Mr. Mayo's original criticism, published in the first number of his "Commentaries," the work to which he refers with so much confidence as containing statements coinciding with those in his "Outlines of Physiology," this word will not be found employed, for ny object whatever, whether in approbation or disapprobation of the views expressed by Sir Charles Bell!

By the term "instinctive," it cannot be doubted that Mr. Mayo meant to express *involuntary* action; indeed, in one or two of the passages of his work,

we may consider that these words were used synonymously. The question, therefore, is, — Did Sir Charles Bell originally represent the portio dura as confined to controlling involuntary motion?

If the reader will turn to the various cases, related in the first chapter, p. 51, of patients affected with paralysis from injury of the portio dura, he will not be at a loss to answer this question. He will perceive that Sir Charles Bell stated that the nerve could control involuntary movements: but he never represented it as confined to these kinds of actions alone. On the contrary, every page, both in that gentleman's own writings and in those of Mr. John Shaw, proves that he considered the portio dura — and correctly considered it — to be a nerve capable of superintending both voluntary and involuntary actions.

Lest any doubt should hang on this subject, I will add some new illustrations, selected, as the former were, from memoirs published before Mr. Mayo joined in these inquiries. The first relate to parts which, no one will refuse to acknowledge, act both voluntarily and involuntarily — I allude to the eyelids: and they are supplied both by branches of the portio dura and the fifth pair. The following note refers to a patient who had the portio dura cut across in an operation — the fifth pair, of course, remaining entire: —

"I recollect," Mr. John Shaw remarks, "that some years ago, in the removal of a tumour from before the ear, the moment the branches of the portio dura were cut, the

girl called out—'Oh, I cannot shut my eye.' The cause of this, though obscure at the time, must now be evident."*

Here no one will deny that the shutting of the eye referred to, was the voluntary closing of the eyelids—not the winking or involuntary actions of these parts.

The next extract is of an analogous description, and is taken from the same paper:—

"The deformity," Mr. Shaw remarks, speaking of a case where the portio dura had been deprived of its functions, "produced by the distortion of the face, and the wasting of the muscles of the paralysed side, are sufficiently vexatious. But the greatest distress is from the loss of power over the eyelids of the same side; for the eye being thus deprived of its natural coverings, during night as well as the day, has been almost destroyed by repeated attacks of inflammation.

"This last consequence is particularly observable in a porter of the name of Garrity, who may be often seen about auction rooms in the west end of London. This poor man, about nineteen years ago, was attacked by a severe pain, accompanied with discharge from the right ear. After a paroxysm, severer than usual, he found, on getting up one morning, that the right side of his face was paralytic. His present condition, and the description which he gives of the progress of the symptoms, prove that the same results followed this paralysis, as in the case of Richardson, already related. But what this poor fellow particularly laments is, that since the day when he was first attacked, he has not

* Med.-Chir. Trans. April 1822.

been able to close his right eye; and well he may: for the constant exposure of the eye to the light and dust has been the cause of so many attacks of inflammation, and consequently of opacity of the cornea, that the eye is now completely destroyed. And this, I fear, will often occur in similar cases; for I have observed that the eye has always become inflamed in those animals in which the portio dura has been cut. It is worthy of remark, that the inflammation has been more severe in the dog and in the ass than in the monkey. One great source of the increase of the inflammation is the purulent secretion from the conjunctiva: this the monkey wiped away with his paw; but it lodged between the eyelids of the dog and of the ass, so as to form an additional source of irritation."—(P. 15.)

Where, then, can any traces be perceived, in these extracts, of its having been supposed that the portio dura presided over the involuntary or winking motions of the eyelids, exclusively.

But, to dispel any further doubts on the question, I will quote another passage, from the same paper, bearing directly on the point at issue.

"We cannot," Mr. John Shaw observes, in treating of the portio dura, "divide the actions of the muscles of the face simply into voluntary and involuntary. Whistling is a voluntary act; but to perform it all the muscles of the face connected with respiration must be perfect in their actions. So it is also in sniffing. The action of the buccinator in mastication, is quite independent of the respiratory organs; and this, we may presume, is the reason why this function of the muscle is not destroyed when the portio dura is cut."

The above sentences Mr. Mayo had the oppor-

tunity of reading, before entering on these inquiries. The next passage is from a memoir that followed immediately after his first paper.

"But the greatest and most marked difference," Mr. Shaw observes, "between the two classes (the 'symmetrical' and 'respiratory'), is discovered by observing their natural functions, and the phenomena of disease. Let us take any one of the superadded nerves—the portio dura, for example,—which goes to the eyelids, nose, and mouth. By a voluntary effort we can close the eyelids, or we can frown; but is either winking, or that action of the eyelids and brows which is so marked during mental emotions, or the closing of the eye during sleep, governed by any power of volition? We can move our lips at will; but is smiling, or laughing, or the actions of the lips during anger, under our control? Or can we whistle or blow, unless the actions of the lips and cheeks are in unison with the respiratory apparatus of the throat and chest? We can turn up the tip of the nose, or we can pull it down; but can we command the muscles of the nostrils during violent respiration? What are we to think of the blindness of those who cannot discover the strong actions produced through this nerve, in the muscles of the nostrils and lips, during that state of apoplexy where all voluntary power is lost? All these complicated functions will be destroyed by cutting the portio dura. Whereas, if we only cut the fifth, that gives branches to all the parts to which the portio dura does, these various faculties will continue unimpaired; while the sensibilities of the parts, and a set of voluntary actions quite distinct from those already enumerated, will be destroyed." *

* London Medical and Physical Journal, Oct. 1822, p. 10.

Who, then, will now venture to assert that it was Sir Charles Bell's object to maintain that the portio dura was exclusively an involuntary or instinctive nerve?

But had Mr. Mayo any intention, in the first number of his "Commentaries," when selecting extracts from Sir Charles Bell's memoir to illustrate his views, of making it appear that the portio dura was considered an instinctive nerve? Let the following note, introduced by Mr. Mayo out of the memoir in question, answer for itself:—

"By the division of a branch of the nerve (portio dura) which passes to the angle of the mouth, a coachman was deprived of the power of whistling." *

These are Sir Charles Bell's words, reported by Mr. Mayo. Now, who will believe that, when transcribing that sentence, Mr. Mayo had any idea that Sir Charles Bell meant by a coachman's "whistling" to give an example of an instinctive action?

It will now be conceded, I trust, that, in treating of the functions of the portio dura, Sir Charles Bell never gave any grounds for causing it to be supposed, that this nerve was an instinctive nerve, or superintended involuntary actions exclusively. It will also be perceived, that when Mr. Mayo reviewed that gentleman's original memoir, in the first number of his "Anatomical Commentaries," he had no idea that such was the nature of Sir Charles Bell's opinions.

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* Mr. Mayo's "Commentaries," No. I., Aug. 1822, p. 118.

It is obvious, on the contrary, that he then knew perfectly what he has since forgotten, that Sir Charles Bell ascribed both voluntary and involuntary power to this nerve.

But, before dismissing this part of the subject, I am bound to present another illustration of the inconsistencies exhibited by Mr. Mayo, in his account of these researches.

It will have been perceived that, in applying a name to the portio dura, it was Sir Charles Bell's object to avoid giving one that involved the idea of its being restricted to controlling voluntary, involuntary, or instinctive actions. Accordingly, he called it the "respiratory" nerve of the face, which bore a reference to the important *organ* of breathing in the higher order of animals. Again, the name he applied to the fifth pair, did not imply that he considered it limited to voluntary, involuntary, or instinctive power, as a motor nerve,—for he called it the "nerve of sensation and *mastication;*" the last of which terms referred likewise to a particular *organ* in the body.

Now, in treating of the fifth pair, in his "Outlines of Physiology," Mr. Mayo appears to have had an insuperable objection to allow that Sir Charles Bell applied the name *mastication* to it; and he has endeavoured to make it appear that he called it the ordinary, or *voluntary* nerve, as the counterpart to the *instinctive* nerve, of the face, the portio dura. But the only illustration he could adduce—it is important to notice—to prove that the fifth was considered a voluntary nerve, opposed to an instinctive nerve, was one

which referred to the action of *mastication*. To satisfy the reader of such being the case, I may be allowed to reinsert the passage referred to :—

" As he (Sir Charles Bell) saw that after the division of one seventh nerve, the nostril ceased to move in breathing (an instinctive action), although the lips continued to be employed in *seizing food* (a deliberate and voluntary action), he concluded that he had obtained by this experiment a proof that the seventh nerve controls the instinctive actions, but not the voluntary actions of the muscles of the face."*

Thus it is directly affirmed by Mr. Mayo that the act of seizing the food is a voluntary act, the reverse of instinctive ; and it is upon these grounds he rests his statement, that Sir Charles Bell contrasted the fifth pair as a voluntary nerve, with the portio dura as an instinctive nerve.

But did Mr. Mayo always entertain the same view, namely, that the act of seizing the food is a voluntary and deliberate action, the reverse of instinctive ? To determine this question, I must refer to a different edition of the " Outlines of Physiology" from that which I have hitherto quoted from. I must refer to the first edition, in which, as I have formerly stated, Mr. Mayo omitted to give any account of Sir Charles Bell's claims to the discoveries in question, and in which he followed the same course that he had previously adopted in the second number of his " Commentaries" — that is, appropriated the discoveries to himself,

* Outlines of Physiology, 1837, p. 174.

without entering into any discussion concerning the prior claims of that gentleman. In that edition, Mr. Mayo actually adduced the act of "seizing the food" as an illustration of *instinct!* I transcribe the passage, taken from the corresponding chapter on Instinct, where he introduces Sir Charles Bell's supposed views, in the succeeding editions: —

"Breathing," Mr. Mayo remarks, "is another action to which we are led by instinct, in order, that when the attention is distracted from this necessity of the body, it may yet sustain no interruption: and throughout life the influence of the instinctive tendency over respiration appears never to be diminished or habitually superseded. *The movements employed in the prehension of food are primarily instinctive,* but are afterwards in a great degree performed upon reflection. Nevertheless, the contact of food resting upon the root of the tongue, always excites an instinctive act of deglutition, which we may suppose intended to ensure the protection of the respiratory organ."*

Thus, we perceive, that originally, before giving an account of Sir Charles Bell's supposed opinions, Mr. Mayo considered "prehension of the food" to be an instinctive, and not a purely voluntary action. But being obliged, subsequently, to explain this gentleman's views, he abandoned that notion, and came to the conclusion that this action was never performed instinctively, but was always done upon reflection, or by deliberation, and volition — and was the very

* Outlines of Physiology. First edition, p. 213.

opposite to instinctive! Nevertheless, we may remark, that if Mr. Mayo had ever observed a chick, newly hatched, picking a grain of corn—or a foal, newly dropped, cropping the grass—he might have been led to acknowledge that his first opinion was quite as good as his last; and that "seizing the food" might very well be called instinctive. On this, however, I must insist, that Sir Charles Bell never even used the word instinct, much less endeavoured to draw a distinction between the voluntary and instinctive actions of the body; and that for Mr. Mayo's representation to that effect, in the last editions of his "Outlines," there is actually no foundation.

The conclusion I think must be, that Mr. Mayo's account of this gentleman's views relating to the portio dura, is just as incorrect as that concerning the fifth pair: and however well the representations regarding both nerves may appear to harmonize with each other, and thus bear the stamp of truth; and however calculated they may be to give rise to the impression that, while investigating the functions of the fifth pair, Sir Charles Bell was ignorant of the distinctions between its roots, the statements concerning both nerves are equally fallacious.

I will now proceed to examine further what Mr. Mayo says concerning the discovery of the distinct functions of the roots of the spinal nerves.

It has been seen that, in the "Outlines of Physiology," Mr. Mayo has given the same account of the particular mode in which he was led to commence his supposed original experiments on the roots of the

spinal nerves, by having previously ascertained the distinct uses of the roots of the fifth pair, as was contained in the second number of his "Anatomical and Physiological Commentaries." But having already examined fully into that representation, in the preceding chapter, I will not offer any further observations upon it here.

As Mr. Mayo, however, at the same time that he gives this account of the mode in which he was led to institute his experiments, imputes certain important errors to Sir Charles Bell, I am bound to direct particular attention to them. I refer to his statement that before he himself, conjointly with M. Magendie, published the true account of the functions of the roots of the spinal nerves, Sir Charles Bell, instead of being aware that the one root was provided for motion, and the other for sensation, entertained the notion that both these functions were bestowed by the anterior root alone : and that the posterior ministered to the growth and sympathies of the parts. It is obvious, that as Sir Charles Bell, in none of his recent publications, admits the truth of this statement, but on the contrary, assumes the credit of giving the first correct description of the functions of these nerves — a description coinciding with that of Mr. Mayo, and without mentioning that gentleman's name—he is open to the charge of having committed an act of injustice towards Mr. Mayo. It is to exculpate him from this implied accusation, that I solicit the reader's attention.

It is important to observe in what work Mr.

Mayo professes to have found the erroneous opinions which he imputes to Sir Charles Bell. If the reader will turn to the passage extracted from the "Outlines of Physiology," at p. 19, he will perceive it stated that these opinions were delivered, on the one hand, in the lectures which Sir Charles Bell gave; and also in his original essay, "The Idea of a New Anatomy of the Brain," printed in 1811.

First, with regard to the lectures. That Mr. Mayo, before anything was published concerning the researches, had attended these lectures, and must, therefore, have had the opportunity not only of hearing the experiments on the roots of the spinal nerves frequently discussed, but, in common with the other pupils of the school, might have witnessed the performance of them, is most true. But as he has not thought proper to adduce any documents whatever to corroborate his statements—such, for example, as his own or fellow-pupils' manuscript notes of the lectures — I feel at liberty to dismiss what he says on this subject, without further observations, and to confine my remarks to what has appeared in print exclusively.

In regard, therefore, to the "Essay on the Brain." This work was written by Sir Charles Bell so far back as 1811. Now it was in August, 1822, that Mr. Mayo published his first memoir—that in which he undertook to criticise this gentleman's new observations on the nervous system. Consequently, if the errors imputed to Sir Charles Bell, and alleged to have been contained in that essay, had any existence, Mr. Mayo had no less than *eleven* years to detect and rectify them before he

composed his criticism. But I beg to ask this simple question—Did Mr. Mayo, in his first memoir, when he had such a full knowledge of Sir Charles Bell's supposed mistaken views, hint that such errors had ever been committed? He did not. In that memoir, Mr. Mayo neglected altogether to treat of the functions of the roots of the spinal nerves! It was not till he wrote the second edition of his "Outlines of Physiology"—*twenty* years after the appearance of the "Idea of a New Anatomy of the Brain"—that he brought these imputations against Sir Charles Bell forward for the first time!

That is to say: in his first memoir, written when, by his own admission, he must have been as well acquainted with that gentleman's opinions as at the present day; and when, moreover, it was his express object to criticise—and that in no friendly spirit—his new observations, he omitted to take notice of the supposed errors: nay, he neglected even to broach the subject of the experiments on the roots of the spinal nerves at all. Again, in his second memoir (July 1823), he refrained from bringing them forward. But in this second memoir he did not omit to speak of the experiments. Their value, which it is obvious he did not recognise at first, he discerned when writing that paper. Accordingly, he adverted to them: but it was for the purpose of claiming the originality of them for himself! It was to make it appear that, by following a particular course of inquiry, he had himself been led, by his own unaided exertions, to originate the same experiments—when, as he observes,

he was ignorant of those performed by M. Magendie! Hence, in this second paper, his object was to participate with the French physiologist in the honour of the discoveries that resulted from the experiments, without adverting to Sir Charles Bell at all. In a succeeding work (the first edition of the "Outlines of Physiology") he adhered to the same course—that is, he declined saying any thing about Sir Charles Bell's supposed errors, and stuck to his conjoint claims with M. Magendie alone. It was not till several years afterwards, when he brought out the second edition of that work, and found himself compelled to admit the priority of Sir Charles Bell's experiments, and to give some account of them, that he charged him with having committed the errors in question.

This mode of proceeding may well excite surprise. But what will be thought, when I remind the reader of another fact? Mr. Mayo represents the experiments on the spinal nerves performed by Sir Charles Bell, as if they had never been laid before the public, except in the "Idea of a New Anatomy of the Brain," until he himself published on the subject, (which was in July 1823.) This work, he also takes care to mention, was unpublished, and only circulated among private friends; thereby leading it to be inferred that it must have been difficult to obtain a perusal of it. But is this statement correct? Sixteen months before the time specified, Mr. John Shaw presented his paper (April 1822) on "Partial Paralysis" to the Medico-Chirurgical Society of London; and in it gave a detailed account of these expe-

riments. Now in this description, no traces whatever are to be found of the errors alleged by Mr. Mayo to have been committed by Sir Charles Bell. On the contrary, every sentence marks distinctly that both this gentleman and Mr. Shaw held identically the same views, regarding each of the roots, that Mr. Mayo claims to have been the first to promulgate. Hence, although it had so happened that Mr. Mayo had never received a copy of the "Idea of a New Anatomy of the Brain"—that unpublished work, so difficult of access; or had never been a pupil of Sir Charles Bell and attended his lectures; or had never had opportunities of witnessing the experiments on the spinal nerves, whether performed by that gentleman, or by Mr. John Shaw, or Mr. Cæsar Hawkins; or had never been put upon the task of making preparations illustrative of the structure of the roots of these nerves as contrasted with the cerebral nerves, but had been educated at a distance from where these researches were pursued—there was nothing to prevent his having made himself acquainted with the true results of the experiments in question, by means of Mr. John Shaw's published description of them, sixteen months before it occurred to him to prosecute these inquiries independently by himself.

I will conclude by giving one or two additional illustrations of the inconsistencies into which Mr. Mayo has been drawn, by the peculiar course he has pursued, in claiming the originality of these experiments.

First: it were a mistake to suppose that, because in his first memoir he neglected to say anything

directly about the experiments on the spinal nerves, Mr. Mayo did not let us perceive clearly enough what he considered, at that time, were the particular opinions held by Sir Charles Bell regarding the functions of these nerves. So far was this from being the case, that in two distinct ways he shewed that he was perfectly acquainted with what this gentleman really believed to be their uses: in other words, was aware that, instead of assigning four separate endowments to the spinal nerves (motion, sensation, the power of controlling the growth, and the power of controlling the sympathies of the parts), Sir Charles only ascribed to them two—motion and sensation.

The first circumstance to which I refer was this—Mr. Mayo did not object to introduce into his criticism the account which Sir Charles Bell had presented of the analogy existing between the spinal nerves and the fifth pair. Now, in enumerating the functions of these nerves, with the view of shewing their analogy, the only endowments that were mentioned were—motion, on the one hand, and sensation on the other. Not a word was said about the power of "controlling the growth," or the power of "controlling the sympathies of the parts," as belonging either to the fifth pair or spinal nerves.

The second was as follows:—In order to controvert the above analogy, Mr. Mayo undertook to shew that the par vagum possessed the same kind and number of functions as the spinal nerves. He even instituted a series of experiments on the laryngeal branches of that nerve, for this purpose. Now, it is interesting to

observe how many functions he considered it necessary to prove belonged to the par vagum, in order to establish that it possessed the same number of functions as the spinal nerves. They were only two. Although he began his experiments by remarking that the par vagum had "many roots," while we know that the spinal nerves have only two—and, had he had any notion of the correctness of the principle that the functions of the nerves correspond with their roots, he would have assigned "many functions" to the par vagum—yet he mentioned only voluntary motion and sensation as belonging to this nerve.

These illustrations, independently of any other evidence, prove that although Mr. Mayo did not mention the experiments on the roots of the spinal nerves, in the first number of his "Commentaries," yet he was aware, when he composed that work, that Sir Charles Bell ascribed to them only motion and sensation.

Finally, let me take an illustration from the "Outlines of Physiology," to illustrate the same point. It must be remembered, that, in this work, Mr. Mayo has not mentioned the fact, that Sir Charles Bell was acquainted with the analogy between the fifth and the spinal nerves. It might not perhaps have agreed with his statement, namely, that the fifth was supposed by Sir Charles to confer only motion and sensation, while the spinal nerves were supposed to minister to four different functions, to have made this acknowledgment; for how could it be affirmed that a nerve, which has only two functions, is analo-

gous to a set of nerves with twice that number? Besides, as I have before remarked, it would have interfered directly with the claims that he puts forward, of having been the first to establish this analogy. But although Mr. Mayo has omitted this important part of Sir Charles Bell's observations — so necessary for understanding his classification of the nerves—he has incidentally shewn, by the term he employs to represent this gentleman's opinions regarding the fifth pair, that he must have been acquainted with the analogy. The word that Mr. Mayo selects to express Sir Charles Bell's supposed views of the fifth is "ordinary." — This nerve, he says, was supposed to confer the "ordinary qualities of nerves," on the parts of the face supplied by the portio dura—a "superadded" nerve. Now, what are we to understand by this word? To what class of nerves in the body did Mr. Mayo allude, as corresponding with the fifth pair in being "ordinary nerves" like it? From the signification of the word, it must have been an extensive class. It could not, therefore, have been the "superadded" nerves of Sir Charles Bell's proposed arrangement. The class of nerves to which he alluded must have been, without doubt, the spinal nerves. Accordingly, by employing the word "ordinary" to designate Sir Charles Bell's opinions concerning the fifth pair, Mr. Mayo has unwittingly shewn that this gentleman must have been aware of the analogy existing between it and the spinal nerves; and as, in treating of the fifth, he did not pretend to shew that this nerve was considered to possess four distinct endowments, so

we have a proof that Sir Charles Bell did not believe that four distinct endowments belonged to the spinal nerves!

In fine, whether we take the fifth pair, the portio dura, or the spinal nerves, and examine into the allegations made by Mr. Mayo, in regard to the functions ascribed to either of them by Sir Charles Bell, we shall find that they are groundless. We shall perceive that the statements in question were never made until Mr. Mayo, in his second memoir on the nerves, had appropriated to himself the correct views concerning these nerves. Calculated as his representation may be to support his pretensions, they are opposed not only to what is found in Sir Charles Bell and Mr. John Shaw's original memoirs, but in an equal degree to what is contained in the memoir which Mr. Mayo himself published, prior to advancing his claims.

CHAPTER V.

EXAMINATION OF M. MAGENDIE'S CLAIMS TO DISCOVERY.

HAVING thus finished what I proposed to bring forward concerning Mr. Mayo, it only remains to offer some observations on the claims to discovery which he advances in favour of M. Magendie; for by the extract from the "Outlines of Physiology" (p. 19) it appears, that Mr. Mayo does not appropriate altogether to himself the credit of the discovery of the distinct functions of the roots of the spinal nerves, but only seeks to share it with the French physiologist.

I propose, in the first place, to inquire whether M. Magendie originated the idea of experimenting on the roots of the spinal nerves, with the view of ascertaining the cause of these nerves possessing sensation and motion conjointly. Secondly, to examine into the alleged correctness of his views concerning the functions of these roots. Finally, to inquire, whether he really was acquainted with the true principle of the discoveries, when he prosecuted his supposed original researches.

First, with regard to M. Magendie's assumed originality in performing the experiments on the roots of the spinal nerves. When this gentleman published his paper (August 1822), detailing his experiments

on the spinal nerves, he represented them as perfectly new; as never having been previously performed, to his knowledge, by any other physiologist. The names of Sir Charles Bell and Mr. John Shaw were not introduced in his paper. Accordingly, so far as these particular nerves were concerned, he appropriated to himself the credit of being the first to adopt the new mode of investigation, namely, that of having recourse to experiments on the roots of the nerves, in order to ascertain the cause of their trunks possessing both motion and sensation. Mr. Mayo, we have also seen (p. 107), in the second number of his "Anatomical Commentaries" (July 1823), encouraged M. Magendie in that course; for after stating that he himself, by investigating the roots of the fifth pair, was led to commence a series of supposed original experiments on the roots of the spinal nerves, he yielded the priority to the French physiologist; omitting, at the same time, to mention either Sir Charles Bell or Mr. John Shaw.

But M. Magendie soon abandoned this pretension; and Mr. Mayo was also forced at length to follow his example. In the succeeding number of his "Journal of Physiology" (October 1822), or two months after writing his first paper, M. Magendie admitted that the experiments on the roots of the spinal nerves, which he had claimed as original, had been performed many years before by Sir Charles Bell; and in the second edition of his "Outlines of Physiology," published about *six years* after M. Magendie had made the above admission, Mr. Mayo allowed that such was really the case.

Let it be observed, however, in what manner they acknowledged the fact; for in doing so, both followed the same course. They selected the "Essay on the Brain," printed by Sir Charles Bell in 1811, and alleged that, before they entered on the subject, this was the only work in which any account of the experiments in question had been given. That is to say, they singled out a work which, on account of its having been *unpublished*, might naturally be supposed to have been inaccessible to them, and represented this work as the only one from which they could have learned anything concerning these original experiments on the spinal nerves before they themselves performed them.

But was this candid? Both gentlemen (M. Magendie as well as Mr. Mayo), when they made use of the "Idea of a New Anatomy of the Brain" in this manner, had ample opportunities of knowing that it was not the fact that the experiments in question had only been related in that privately circulated work, when they repeated them. They ought to have known, that although Sir Charles Bell had not given, at that time, any additional account of the experiments, yet a full description of them had been published by Mr. John Shaw. Five months before either M Magendie or Mr. Mayo had written a single original memoir on the nervous system, Mr. John Shaw's paper on "Partial Paralysis" (April 1822), had been read to the Medico-Chirurgical Society; and it contained, as we have already seen, not only a notice of the experiments related in the "Essay on the Brain," but an account of additional experiments performed subsequent to the

appearance of that work. Hence, even if the "Idea of a New Anatomy of the Brain" had never been written, there was no excuse for M. Magendie or Mr. Mayo representing their experiments as original.*

It has next to be asked, Suppose M. Magendie had never seen either the "Idea of a New Anatomy of the Brain," or Mr. Shaw's paper on "Partial Paralysis," was that a reason for his omitting to mention Sir Charles Bell's name, in describing the experiments on the roots of the spinal nerves?

To answer this question, it is necessary to be aware, that although the paper on the spinal nerves was the first in which M. Magendie treated of the nervous system as an original inquirer, yet it was not the first that he had published at that time on the discoveries. On the contrary, by referring to his "Journal of Physiology," it will be perceived that, as early as October 1821, he had commenced giving an analysis of all the successive papers on the nervous system that were published in this country. These amounted, before he printed his own memoir, to four in number. Now, in the first, which was an abstract of the general views

* It deserves to be mentioned, that in acknowledging the priority of the experiments on the spinal nerves related in the "Idea of a New Anatomy of the Brain," M. Magendie clearly shews that he did not suppose that Sir Charles Bell had committed the errors which Mr. Mayo subsequently imputed to that gentleman. On the contrary, by the extracts which he selected from the work, as well as by his comments, he proves, that he understood Sir Charles Bell's meaning as Sir Charles intended it should be understood; and not as Mr. Mayo, in his "Outlines of Physiology," afterwards chose to interpret it.

announced by Sir Charles Bell, derived avowedly, in part, from Mr. John Shaw's "Manual of Anatomy" (September 1821), and in part from personal communications with the author of that work himself (who happened to be on a visit to Paris in the autumn of that year),* we find the following sentence in M. Magendie's essay:—

"Mr. Charles Bell has endeavoured to discover the reason of the complexity of the nervous system in man. He has undertaken to prove, *that nerves possess distinct functions in correspondence with the portions of the brain or spinal marrow from which they originate.* These views the author has established, both by observations on the nerves in man and the lower animals; and he has confirmed them by a series of most ingenious experiments.'

The above passage is to be met with in the very second paragraph of M. Magendie's communication; and it cannot be doubted that it expresses, as clearly as it would be possible to convey any one's meaning, the fundamental principle of the new researches. It was for the purpose of confirming the law, that "nerves possess distinct functions in correspondence with the portions of the brain and spinal marrow from which they arise," that, first, the experiments on the roots of the spinal nerves, and afterwards those on the cerebral nerves, were performed.

For example, putting out of view, for the present,

* The part of the "Philosophical Transactions" containing Sir Charles Bell's first memoir (July 1821), had not been published when Mr. Shaw visited Paris.

the experiments on the spinal nerves, what was the main object of the different papers on the cerebral nerves translated by M. Magendie, if it were not to illustrate the distinction existing between the portio dura and fifth pair, resulting from the difference between their roots? The reason for motor power, independently of sensation, having been ascribed to the former, was, that it arose from the brain by a single root; and could not, therefore, according to the principle, minister to more than one function. Again, the reason why the fifth pair was distinguished from the portio dura, and supposed to confer both motion and sensation, was, that instead of arising from the brain by a single root, it had two distinct origins: one provided for motion, and the other for sensation. When we consider, therefore, that M. Magendie, from having translated the papers on these subjects published in this country, must have been aware that such were the objects for which so great importance was attached to the fifth pair and portio dura, is it not surprising that, in communicating the results of his experiments on the roots of the spinal nerves, he should have omitted to draw attention to what had been established concerning these cerebral nerves?

But another ground for our disappointment in M. Magendie's conduct in this respect must not be passed over. Besides the reasons just noticed, for so much importance being attached in the original memoirs to the portio dura and fifth pair, there were certain others which ought not to be omitted. These

two nerves, in Sir Charles Bell's proposed arrangement, occupied distinct places. The portio dura was the representative of one particular class, and the fifth pair was the representative of a different class. Now, without adverting further to the nerves with which the portio dura was associated, that is, the "superadded class," what were the nerves that formed the class in which the fifth pair was included? They were the spinal nerves. The spinal nerves, together with the fifth pair, constituted that extensive and important class which Sir Charles Bell called the "regular," or "original" class. That is to say, out of the ten or twelve cerebral nerves, the fifth pair was selected as the only one which in the structure of its roots, in the nature of their origin from the columns of the brain, and the functions it bestowed, bore a resemblance to the spinal nerves. Consequently, the fifth pair being thus the representative in the head of the spinal nerves in the body, no one will refuse to allow, that the observations and experiments brought forward to illustrate its particular functions, had a direct reference to those of the spinal nerves. To shew that M. Magendie was not unacquainted with the classification here referred to, let me quote the following passage :—

"From this work we learn," M. Magendie remarks, alluding to the "Manual of Anatomy," "that by observing how the two great divisions of the brain are prolonged into the columns of the spinal marrow, and also how the nerves arise from these, two distinct systems of nerves

can be proved to exist in the body. The characters of these two classes are thus explained by Mr. Shaw. Taking the first, the columns which compose the spinal marrow are divided into those which are continued from the cerebrum and those which are continued from the cerebellum. Now, every nerve that belongs to the regular system arises by two separate roots; one from the anterior column, and the other from the posterior column. In this class are included the fifth pair, the suboccipital, seven cervical, twelve dorsal, five lumbar, and five sacral nerves; so that, on the whole, there are thirty-two perfect, regular, and double nerves, common to man and the lowest animals, and each possessing motion on the one hand, and sensation on the other."*

Who can question, therefore, after reading the above passage, that the observations on the fifth pair, which M. Magendie transferred, in his own language, from Sir Charles Bell's and Mr. John Shaw's writings to his Journal, might have suggested the idea of performing the experiments on the roots of the spinal nerves, even if he had not read Mr. John Shaw's paper on "Partial Paralysis?" Such was the more especially to be looked for, when we consider that the original account of the resemblance between the fifth pair and spinal nerves, in the "Manual of Anatomy," was accompanied with an engraving which represented the fifth pair associated with the spinal nerves, as the only cerebral nerve that bore that resemblance. Again, we have to reflect, that Mr. John Shaw, the author of that work, had only

* Journ. de Phys. Expérim., Oct. 1821, p. 388.

a short time before his visit to Paris, namely, in March 1821, been engaged in repeating the experiments on the roots of the spinal nerves, before the pupils of the school of Great Windmill Street; and a few months afterwards (April 1822) he delivered his account of the same experiments to the Medico-Chirurgical Society of London. And M. Magendie has admitted,* that at the same time that this gentleman presented him with the copy of his "Manual," he took the trouble to perform the experiments on the fifth pair and portio dura, in his presence, to illustrate Sir Charles Bell's views.

I cannot, therefore, help concluding, that when M. Magendie brought forward his experiments on the roots of the spinal nerves, as having been originally devised by himself, and never previously performed, he had enjoyed ample opportunities, not only of knowing directly that the same experiments had been frequently performed in this country; but of being acquainted with the general principle, which had led Sir Charles Bell first to take the roots of these nerves under his examination, and then to pass to the nerves of the brain, for the purpose of investigating them also, in reference to their roots. And such being the case, I cannot but entertain the opinion that he did not act with becoming candour in claiming originality for his experiments.†

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* See his paper, Oct. 1821.

† Without venturing to draw any inference from the fact, I may be allowed to point out a somewhat remarkable circumstance connected with the republication, by M. Magendie, of the different memoirs on

Having thus dismissed the question of originality, in regard to adopting the mode of experimenting on the roots of the spinal nerves to ascertain the cause of these nerves possessing distinct functions, I may pass to the next head of inquiry; namely, whether M. Magendie really expressed the opinions concerning the functions of each of the roots, that are commonly attributed to him. I feel called upon to request attention to this subject, because in no part of the history of the discoveries does there appear to be such a remarkable misapprehension existing.

Mr. Mayo, we have seen, alleges, in a positive

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the nerves that originally appeared in this country. The number of these distinct papers, translated and introduced into the "Journal of Physiology," was four. Now it will be discovered, by comparing the relative dates of the original papers themselves with those of the translations, that scarcely any interval of time elapsed between the period of their publication in this country and Paris. However, the point to which I beg to call attention is the following: — This process of reprinting in Paris what had been published in London, came to an abrupt termination at a remarkable time. It happened that none of the four publications referred to contained a detailed account of the experiments on the roots of the spinal nerves. Now, when the paper entitled "On Partial Paralysis," in which Mr. John Shaw had introduced a description of these experiments (and which was the next in order to the last that had been translated by M. Magendie), was published, no report of it whatever was given in the "Journal of Physiology!" Moreover, it has never yet been alluded to by M. Magendie in any of his works. Nevertheless, the reader is aware, that between the date of this paper's being read to the Medico-Chirurgical Society and M. Magendie's memoir on the spinal nerves, an interval of five months elapsed; and between the date of its publication in the society's "Transactions" and that memoir, there was an interval of two months: ample time for him to have translated, or at least noticed it, in his Journal.

way, and without any proof, that it was in order to establish that the anterior roots bestow motion exclusively, and the posterior roots sensation exclusively, that M. Magendie published his supposed original experiments on the spinal nerves. And such, without doubt, is the generally prevailing opinion in the profession and the scientific world, in regard to this physiologist's share in the researches. But nothing can be further from the truth than such a representation.

The views of the functions of the roots of the spinal nerves which M. Magendie sought to establish by his experiments were these: that to each root respectively belonged both motion and sensation! He affirmed,

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To confirm what is above stated, let me give the comparative dates of the different productions referred to: —

1. Mr. John Shaw's "Manual of Anatomy" was published in September 1821. In October following, the abstract, before noticed, was composed from it, and inserted in the "Journal of Physiology."

2. Sir Charles Bell's first paper to the Royal Society, read in July 1821, was published in October of that year. M. Magendie gave a brief account of this paper in his Journal for January 1822.

3. Mr. John Shaw published his paper "On the Difference between the Functions of the Nerves of the Face," in the "Quarterly Journal of Science" for January 1822. In the "Journal of Physiology" for the *same* month, there is a full translation of this paper, by M. Cairns.

4. The succeeding paper by Mr. John Shaw, treating of the same subject as the former, was published in the "Quarterly Journal of Science" for April 1822. As in the former instance, a translation of this paper appeared in M. Magendie's Journal for the *same* month.

The next paper in succession, published in this country, was that on "Partial Paralysis," by Mr. John Shaw, containing the account of the experiments on the roots of the spinal nerves. But, as I have mentioned, we do not find any notice, far less any translation, of this paper in M. Magendie's Journal.

that although the anterior root governed the muscles, yet it was a sensitive nerve at the same time; and although the posterior root conferred sensation, yet it was a motor nerve at the same time. In short, the true object of his investigations was to establish that motion and sensation resided conjointly in each of the distinct roots of the spinal nerves.

To prove the accuracy of this statement, let me select some extracts from M. Magendie's works.

It may be allowed that, in his first memoir (August 1822), he did not deliver his views quite so distinctly as I have represented above. Yet it will be seen, that at the close of his paper, when summing up his observations, he was far from affirming positively that each root had a distinct or appropriate function. His experiments, he would only venture to say, "made it appear that the posterior roots were more particularly provided for sensation, while the anterior were more particularly provided for motion."

But in his second memoir (October 1822), he expressed his opinions decidedly enough. What can be more explicit than the following?—

"Almost every time," M. Magendie observes, "that I irritated the posterior roots, the particular muscles to which the nerves were transmitted were thrown into a state of contraction . . . When a posterior root was cut across, it gave rise to a movement in the whole limb to which it was distributed. On repeating the same experiment with the anterior roots, analogous effects were produced, but of an inverse kind; that is to say, when the latter roots were pinched or pricked, powerful convulsive actions of the muscles followed, while

the signs of sensibility were faintly indicated. Accordingly, although these facts tend to confirm my preceding statements, yet they clearly shew that the power of giving sensation does not belong exclusively to the posterior roots, nor the power of giving motion exclusively to the anterior roots."*

The next extract is to the same effect. He is speaking of the results of applying a galvanic current to each of the roots :—

"Whichever root of the nerves," he observes, "was selected for experiment, contractions of the muscles were produced. In general, however, the contractions occasioned by applying the current to the anterior roots, were stronger and more distinct than those obtained from the posterior roots."†

In a paper of later date,‡ we find the following account of the effects produced by injuring the posterior column of the spinal marrow :—

"Upon irritating this part, signs of acute pain were manifested; and it was observed, that the particular muscles which receive their nerves from this portion of the spinal marrow, were thrown into violent contractions: these contractions were confined to the side of the body corresponding with the column which was irritated."

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* "Seulement ils semblent établir que le sentiment n'est pas exclusivement dans les racines postérieures, non plus que le mouvement dans les antérieures."—*Journ. de Phys. Expérim.* Oct. 1822.

† Journal de Physiologie Expérim. Oct. 1822.

‡ Ibid. April 1823.

The last quotation is from a work on the Nervous System, published jointly by M. Magendie and M. Desmoulins :—

"If we apply a galvanic current," it is observed, "first to a posterior, and then to an anterior root, having previously cut them across, and thus insulated them from the spinal marrow, we shall be sure to have contractions of the muscles produced with both the roots. But the contractions consequent on irritating the anterior roots, are in general more forcible and perfect than those which follow irritation of the posterior roots . . . By cutting across the posterior roots, we shall cause the animal to kick out with the corresponding leg. By pinching or pricking the anterior roots, a different result will follow : convulsions of the muscles will take place, while the indications of pain will be indistinct. On the whole, we are forced to conclude that there is no absolute distinction between the functions possessed by the two roots." *

Could any thing be more unequivocal than these opinions? Or could any views differ more widely from those for which it is usual to bring forward M. Magendie's name in connexion with these discoveries? As it has been frequently stated above, the great principle of the discoveries is to this effect,—that for a nerve to possess two such distinct functions as sensation and motion at once, it must have two distinct roots, or be composed of filaments derived originally from different parts of the

* "L'isolement des deux propriétés dans chacun des ordres de racines, n'est donc pas absolu."—*Anat. de Système Nerv., par Magendie et Desmoulins.*—(P. 777.)

spinal marrow or brain; one set of filaments coming from the seat of sensation, and the other from the seat of motion. But here it is obvious that M. Magendie virtually denies such a principle to exist. He leads it to be supposed, on the contrary, that the anterior roots of the spinal nerves, although they proceed from a single column of the spinal marrow, are endowed with sensation as well as motion; and that the posterior roots, although they proceed also from a single column alone, enjoy motion as well as sensation. Consequently he leaves it to be inferred, that the one root may serve as a substitute for the other; or that with one set of roots alone, whichever it be, we may possess both motion and sensation conjointly.

Let me point out another legitimate conclusion from his experiments. They lead to the opinion, that it is possible for a single nerve to convey along its fibrils two distinct nervous impulses, which run in a contrary course to each other, at the same instant of time. The impulse carried along a nerve, in order to give rise to the impression of sense, it cannot be questioned, travels in the opposite direction to that which gives rise to a contraction of the muscles. As the impression, in the production of a sensation, is communicated originally to the extremity of the nerve expanded upon the integuments, and is conveyed from that to the sensorium, we are sure that the impulse, of whatever nature it may be, travels from without inwardly; while, on the other hand, as the mandate of the will, destined to call the muscles into action, originates in the brain, we cannot doubt that the impulse which influences the muscles is propa-

gated from within outwardly, or in the reverse direction compared to the impulse producing a sensation. Hence, if we confide in the correctness of M. Magendie's observations on the roots of the spinal nerves, we must be forced to conclude that each of these roots has the extraordinary property of conveying along its fibrils an influence that travels inwardly, and another that travels outwardly, at the same moment; and that each column of the spinal marrow, the anterior as well as the posterior, can receive an influence which comes from without, and give out another which is to be conveyed externally, at the same instant. But these are suppositions, bearing upon their very face so much the appearance of error, that it is impossible to admit them. They are so improbable, that I have already had occasion to remark that they were considered by Sir Charles Bell, in his "Idea of a New Anatomy of the Brain," as affording *prima facie* evidence that the nerves of sensation must necessarily be distinct from those of motion.

It may not be without its use, however, to inquire how M. Magendie was led to arrive at these conclusions in reference to the roots of the spinal nerves; or, I may safely say, to fall into these errors? It cannot be doubted that it has been from trusting implicitly to the results of experiments made within the vertebral canal alone, and refusing to take advantage of those performed upon the cerebral nerves. We have already had occasion (p. 44) to notice what violence was necessarily inflicted in the operation of laying bare the roots of the spinal nerves for experiment; and how little

reliance could be placed on observations made on an animal that had suffered all the injury implied in exposing them; more especially in reference to the degree of sensation residing in either of the roots.* Accord-

* I subjoin an extract from M. Müller's work on physiology, now in progress of being translated, for the purpose of shewing how unsatisfactory all the experiments on the roots of the spinal nerves, by whomsoever performed, have hitherto been; and how essential it is to combine other experiments with them to obtain any certain conclusions. This is the sole object for which I insert the quotation: for the reader will perceive, that in the account which M. Müller gives of these investigations, he has committed the most unaccountable errors—saying that Sir Charles Bell had never performed the experiments on the roots of the spinal nerves before M. Magendie entered on them; and omitting to mention Mr. John Shaw's previous description of these experiments, in his paper on "Partial Paralysis:"—

"The fact that the same nerves (?) supply the body with sensitive and motor power, and that one of these functions of a nerve may, in consequence of paralysis, be lost while the other is preserved, is one of the most important in physiology. Sir Charles Bell first conceived the ingenious idea that the posterior roots of the spinal nerves, which have upon them a ganglion, are the source of sensation; the anterior roots, of motion; and that the primitive fibres of these roots, after their union, are mixed, and thus distributed for the supply of the skin and muscles. This view he proposed in 1811, in a treatise entitled 'An Idea of a New Anatomy of the Brain, submitted for the Observations of the Author's Friends.' Eleven years later, *the same theory was advanced by M. Magendie, who, however, has the merit of having first subjected it to the test of experiment in the case of the spinal nerves.* M. Magendie's results were only approximative. He asserted that the posterior column of the spinal cord, and the posterior roots of the nerves, supplied sensation principally; the anterior, principally motion; but that the latter were not wholly devoid of sensitive power. Thus, in his experiments, the application of galvanism to the posterior roots of the spinal nerves, after their separation from the spinal cord, excited contractions of the muscles, though these were but feeble; while the same stimulus applied to the

ingly, we may be allowed to surmise, that if M. Magendie had followed the example originally set before him by Sir Charles Bell, of examining the nerves of the brain with the view of comparing the results of his observations on them, with those obtained by experimenting on the spinal nerves, he would most pro-

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anterior roots gave rise to violent muscular spasms.* These experiments performed on the higher animals are the most cruel that can be imagined. The extensive wound necessary for laying open the spine in sufficient length to enable the operator to divide the roots of all the nerves which go to the posterior extremities, produces a great shock to the system, is attended with very great hemorrhage, and death inevitably follows in a very short time, before satisfactory results can be obtained. Great, therefore, as was the interest which Sir Charles Bell's theory, thus newly illustrated by M. Magendie's experiments, excited, a satisfactory confirmation of the results was still wanting. Béclard was the only physiologist who spoke on the point in a decided tone; but it was, at the same time, in too superficial and unconvincing a manner.† Fodéras's experiments were accompanied with such contradictory symptoms, that it is inconceivable how he could put them forth as confirmatory of M. Magendie's observations. Bellingeri obtained totally different results, and arrived at the conclusion that the internal grey substance of the spinal cord supplies sensation; the white fibrous substance, the motor power: and that the anterior columns and the anterior roots are destined for the motions of flexion, and the posterior for those of extension. The above experiments were carefully repeated in Germany, by M. Schoeps, on many animals;‡ but the results obtained were very doubtful and uncertain. I had myself, as early as 1824, during my stay in Berlin, performed them without success. More recently, being engaged in researches on the nervous system, the desire of ascertaining the real facts of the case induced me to perform a new series of experiments on rabbits, and on a

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* Journ. de Physiol. ii. 276. Compare Desmoulins et Magendie, "Anat. et Physiol. des Systèmes Nerveux." Paris, 1825, p. 777.

† Elém. d'Anat. Générale. Paris, 1823, p. 668.

‡ Meckel's Archiv. für Anat. et Physiol. 1827.

bably have formed very different conclusions from those he has actually announced.

In reference to this point, I beg to remind the reader, that it was not until Sir Charles Bell had extended his inquiries from the spinal to the cerebral nerves, and especially until he had ascertained the

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different plan. The mode in which the investigation had previously been conducted is evidently deceptive, from the circumstance that many animals, especially rabbits, are so much frightened by the first steps of the experiment, that the most violent irritation of the skin, not even pinching it and cutting it, causes them to manifest any pain. How then can we, in the short time which an animal lives after the spinal canal is opened, determine with certainty whether it has sensation or not?"

After dwelling further on the uncertainty attending these experiments, M. Müller states that he was subsequently led to institute them on frogs; by which he obtained a satisfactory confirmation of Sir Charles Bell's views:—

"In deciding a question absolutely," he continues, "no half results, no approximatives, are sufficient. The theory of Bell was extremely ingenious; but its truth appeared to me still to require demonstration. Even M. Magendie had not decided it satisfactorily. I thought it perhaps impossible to decide it with certainty in the higher animals. This opinion, that the theory of Bell had not been properly established by experiment, was held by Professor E. H. Weber also. The happy thought at length occurred to me of performing the experiment on frogs."*

It is to be observed, that these experiments on frogs evince the greatest care; and no one doubts their perfect accuracy. But it is remarkable, that before making so many objections to the investigations which preceded his experiments, M. Müller should not have adverted to the confirmation which Sir Charles Bell's views on the spinal nerves received from examining the functions of the cerebral nerves. This is a question altogether passed over by M. Müller; but it is one on which both Sir Charles Bell and Mr. John Shaw, as will be further seen in the text, have laid the greatest stress.

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* Müller's Elem. of Physiol. translated by Baly, vol. i. p. 640.

proper functions of the fifth pair and portio dura, that he ventured to speak decidedly of the results of his experiments on the roots of the spinal nerves. It was only when he had found, by experimenting on the portio dura, that this nerve, in correspondence with its arising by a single root, had only a single function; while the fifth pair, in correspondence with its arising by a double root, like that of the spinal nerves themselves, had both motion and sensation, proportioned to the distribution of its distinct roots; that he threw away all doubts as to each root of the spinal nerves being limited to a single and appropriate function. By observing, for example, in the fifth pair, that it was the ganglionic root, or that which resembled the posterior roots of the spinal nerves, that gave sensation, and that it was confined to this office, he inferred that the posterior roots of the spinal nerves were exactly of the same nature. The following passages bear upon this point, and I may therefore introduce them here:—

"In every age," Sir Charles Bell observes, "the brain has been considered a common sensorium, and all the nerves to be capable of conveying sensation, unless when they had ganglions. These notions were obviously founded on the mistake, that the same nerve served different purposes, and that a fluid moved in the same tube, outwards to stimulate the muscles, and inwards to convey sensations of external impressions. So inconsistent are these opinions with the structure of the frame, that the simplest dissection proves them to be false. So far is it from being true that ganglions cut off sensation, that I have ascertained and proved, by

experiment, that all the nerves, without a single exception, which bestow sensibility, from the top of the head to the toe, have ganglions on their roots: and those which have no ganglions are not nerves of sensation, but are for the purpose of ordering the muscular frame.

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"In this view of the fifth nerve, I have not touched upon its resemblance to the spinal nerves. But if we had ascended from the consideration of the spinal nerves to the nerves of the head, we should then have seen that the fifth was the spinal nerve of the head; that it had a ganglion at its root, a double origin, and from its power over the muscles of the jaws and mastication, that it was a double nerve in function, being that nerve which bestows sensibility, at the same time that it sends branches to the original muscles; that is to say, to that class of muscles which are common to animals in every gradation. In all these respects it resembles the spinal nerves." *

Again, from Mr. John Shaw we learn, that by comparing the anterior roots of the spinal nerves, in a similar manner, with certain nerves of the brain, resembling them in their origins, Sir Charles Bell confirmed his view, that these anterior or non-ganglionic roots were destined for conferring motion.

"I may observe," Mr. John Shaw remarks, "that previous to having made these experiments (on the roots of the spinal nerves) Mr. Bell entertained the opinion, that the anterior column of the spinal marrow was different in function from the posterior; and that through it the simple

* Philosophical Transactions. June 1823.

voluntary power of moving particular parts was conveyed. He deduced this from observing that the two nerves, which are generally supposed to be purely motors — viz. the third or motor oculi, and the ninth or motor linguæ — arose from the anterior fasciculus." *

Indeed, in reference to the ninth pair, which no one hesitates to allow is a motor nerve alone—its mode of arising from the corpus pyramidale is so exactly like that of the anterior roots of the spinal nerves, that had there been a posterior root connected with it, we might have considered it as the anterior root of a true spinal nerve.

But M. Magendie, in treating of the functions of the spinal nerves, did not make any attempt to elucidate the question as to whether these roots possessed both motion and sensation conjointly, by referring to the cerebral nerves. And this leads me to direct attention, in the next place, to the views entertained by him concerning some of the nerves of the brain, for the purpose of answering more fully the third question which I proposed to examine; namely, How far M. Magendie was acquainted with the correct principle of pursuing the researches when he first commenced them?

I will take what he said of the functions of the portio dura, in the first place. This nerve, I must repeat, was brought forward by Sir Charles Bell as a favourable example of a nerve which, in virtue of its arising from the brain by a single root, could only

* Medical and Physical Journal. Dec. 1822.

minister to one function. It was shewn by experiment, and by a numerous series of cases of partial paralysis of the face in the human subject, that when the portio dura was destroyed, motor power alone was lost; that no diminution of the sensibility of the face took place. In accordance with this view, of its being limited to bestowing motion, Sir Charles Bell also remarked, in his original paper, when contrasting it with the fifth pair, that when exposed in the living animal, and cut across, no indications of pain were manifested.

"The cutting of the fifth pair," he said, "gave pain in a degree corresponding with our notions of the sensibility of nerves; but on cutting the portio dura, it was not evident that the animal suffered pain at all."

In reviewing this communication, however, M. Magendie objected to the above statement; and made the following observation:—

"We have repeated," he remarks, "the same experiment; but the animal testified, at the moment of cutting through the nerve (the portio dura), by its struggles and cries, that the nerve was acutely sensible. Although we do not doubt the correctness of Mr. Bell's statement, yet one ought to be especially guarded about coming to any general conclusion on such a subject. We have frequently met with animals in which we might prick, cut across, or tear the different nerves, without their appearing at all sensible of the injury; so that, judging from these experiments, we should have declared that the nerves were devoid of sensibility. The animals employed by Mr. Bell were probably of that description."

The futility, I may safely say, the utter folly, of this objection, is surprising. Sir Charles Bell's statement was to the effect, that when, *in the same animal*, first the fifth pair, and then the portio dura, were cut across, the greatest difference was manifested in their sensibility. If M. Magendie's criticism were of any value, and the degree of sensibility in the nerves depended on the peculiar temperament of the animal, is it not obvious that the same effects exactly would have followed with both the nerves—that the same pain, and no more, would have been exhibited in experimenting upon the fifth pair, as in experimenting on the portio dura? But this is a fact he has not attempted to demonstrate.

Leaving that, however, out of the question, could anything have betrayed more clearly M. Magendie's misapprehension of the true mode of examining the subject, than the remarks I have quoted? Supposing it were the case, that, on exposing the portio dura in a living animal, he found it endowed with sensibility, as well as the power of bestowing motion; was it the true mode of accounting for its possessing sensation, to say that it depended on the general nervous temperament of the animal? Let me contrast this explanation with what we find in one of Mr. John Shaw's papers, written shortly afterwards, when engaged in treating of the same experiment—namely, that of cutting across the portio dura, with the view of ascertaining how far it was endowed with sensibility:—

"Some difficulty," he remarks, "may arise from our mistaking compound nerves for simple ones. On one occa-

sion I was nearly drawn into a mistake, by not observing that a small filament of the fifth nerve entered into the portio dura on the face. There may be branches of simple sensation combined with those of the eighth and ninth."*

Hence it is perceived, that when Mr. Shaw, in experimenting on the portio dura, found that pinching it gave rise to pain, he did not fall into the error of supposing that this sensibility was due to the portio dura itself, far less that it depended on any peculiar nervous habit of the animal. He concluded that it was owing to certain filaments of the fifth pair—the sensitive nerve of the head, being embraced in the same sheath with the portio dura—and to these filaments being injured in the experiment, in common with the portio dura.

M. Magendie's observations would have led to the supposition, that the single nerve—the portio dura—could combine the two functions of giving motion and sensibility; and if he had been successful in establishing that fact, it might have added a new proof in confirmation of his view, that each root of the spinal nerves possessed the same compound properties. But Mr. Shaw, we have seen, relying on the principle first enunciated in the "Idea of a New Anatomy of the Brain," that when a nerve ministered to two such distinct offices, it was a sign that it was composed of two distinct sets of filaments—examined the anatomy of the portio dura with care, and discovered that, at the part

* Medical and Physical Journal. October 1822, p. 10.

of its course where it was exposed for experiment, this nerve was not in fact a single nerve, but was partly formed of filaments derived from the fifth pair, the nerve of sensation.

The next illustration I have to offer of M. Magendie's indifference to the true principle of conducting these researches, relates to the fifth pair.

It has formerly been shewn, that the chief object Sir Charles Bell had in view, in treating of the fifth pair, was to prove that, in correspondence with its arising from the brain by two distinct roots, like the spinal nerves, it possessed both motion and sensation; and that it was the only cerebral nerve which had these two functions combined. But it had also to be pointed out, at the same time, that, in his original experiments designed to confirm this view, he was not successful. The experiments which he adduced to demonstrate its power of bestowing sensation were satisfactory; but such was not the case with those intended to prove that it possessed *motion*. From having taken the infraorbitary branch, which is a purely sensitive branch, as the subject of his experiments, under the impression that it combined motor power with sensation, he failed in obtaining the proof that he wanted.

It was M. Magendie (see p. 98) who first detected this imperfection in the experiments. On repeating the experiments on the infraorbitary branch, this physiologist observed that Sir Charles Bell was correct in ascribing sensation to it; but he could not perceive that it exercised any control over the muscles. Conse-

quently he rendered null the only experimental proof that had been addueed to establish that the fifth pair was a *motor* as well as a sensitive nerve.

But after having done this, M. Magendie abruptly ceased his inquiries; that is, as soon as he had invalidated the only experiment by whieh he knew Sir Charles Bell and Mr. Shaw intended to prove that the fifth could give motor power. Whence it is fair to allege, that, from his relinquishing the subjeet at that point, he was of opinion, that notwithstanding the fifth pair had two roots, it was only endowed with sensation—was nothing but a mere sensitive nerve.

I give, however, too much credit to M. Magendie, in speaking of him as if he were acquainted with the faet that the fifth pair originated by two roots, or resembled the spinal nerves. It will be found that, notwithstanding the importanee attaehed, in every memoir, both by Sir Charles Bell and Mr. John Shaw, to the anatomieal strueture of the fifth pair, and to its arising by two roots—a ganglionie and a non-ganglionie—M. Magendie, in none of his subsequent papers, when treating of the fifth pair, ever introdueed a remark as to its arising by two roots, or to its possessing motion as well as sensation, or to its being like the spinal nerves!

It has already been shewn (p. 99), that as soon as it became apparent, by the repetition of the experiments on the infraorbitary braneh, that an experimental proof was still wanting of the fifth being able to give motion as well as sensation, Mr. John Shaw instituted a new set of experiments to supply that defieiency. He

selected the third division of the nerve, at the point where it escapes from the base of the skull, carrying with it the motor root. By exposing that division in the living animal, and irritating it with the forceps, he obtained the most satisfactory evidence of its controlling the muscles of the jaws, as well as bestowing sensation. But in none of his communications on the functions of the nerves, which followed Mr. Shaw's paper, did M. Magendie take any notice of these new experiments, or of the cause that gave them so much interest!

A still more remarkable fact, however, remains to be mentioned. It happened that, two years after the time referred to, M. Magendie commenced a new and distinct series of experiments upon the fifth pair (July 1824). In these experiments, it cannot be doubted, that he cut across, on many different occasions, both roots of the nerve, at once — the motor as well as the sensitive. Now, in none of his descriptions of these experiments will it be found, that he ever gave a hint that the fifth pair arose by two roots, or was a motor as well as a sensitive nerve!

This is a subject that deserves a little more detail. From examining the distribution of the fifth pair in the head, and perceiving that several of its branches entered into the different organs of the senses; and also from having certain doubts, it may be presumed, as to whether the olfactory, the optic, and auditory nerves, were transmitted to the nose, eye, or ear, in such a manner as to be capable of receiving adequately the impressions conveyed to these organs;

M. Magendie took up the notion that it was quite a mistake — an ancient prejudice — to suppose that smelling, seeing, or hearing, depended on the olfactory, the optic, or the auditory nerve; and he entertained the bold project of demonstrating that these special senses all depended upon the branches of the fifth pair. In order to accomplish this object, he instituted a series of experiments on the roots of the nerve, within the cranial cavity.* By perforating the skull, and inserting a bistoury between the base of the brain and that of the skull, he contrived to cut across the fifth pair, just at its origin from the side of the pons Varolii: and this he did, in the greatest number of

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* To shew that I am not unjust to M. Magendie, I subjoin the concluding sentences of his papers, wherein he sums up the results of his experiments:—

"S'il se confirme que l'odorat appartient à la cinquième paire, il restera à rechercher quels peuvent être les usages des nerfs et des lobes olfactifs. Rien jusqu'ici ne semblerait mettre sur la voie. Ce serait, dans ce cas, des parties à ajouter à toutes celles qui dans le système nerveux ont des fonctions entièrement ignorées."—*Journ. de Physiol. Expér.*, Mai 1824, p. 175.

"La vue paraît être, si non perduc entièrement par la section du nerf, (cinquième paire), du moins très affaiblie. . . . . Des que les deux nerfs (cinquièmes paires) sont coupés sur un animal, il semble aveugle, et sa démarche est des plus singulières: il ne marche que le menton appuyé fortement sur le sol, poussant ainsi sa tête devant lui, et s'en servant comme d'une guide, ou comme l'aveugle de son baton."—P. 180.

"Je crois avoir remarqué, que la section de la cinquième paire entraine aussi la perte de l'ouïe: cela serait d'autant moins extraordinaire, que dans beaucoup d'animaux le nerf acoustique n'est évidemment qu'une branche du trifacial. Si ce dernier résultat est exact, tous les sens seraient donc sous l'influence de la cinquième paire, et la théorie générale des sensations devrait donc être réformée."—P. 182.

his experiments, on both sides of the head at once, so that the animal had both its fifth pairs totally destroyed at the same time. Hence the question arises, Did M. Magendie, after having performed these experiments, offer any observations as to the effects produced by his operations, on the muscles of the jaws? No one who reflects on the closeness with which the filaments composing the motor root adhere to those of the sensitive root, in their passage from the pons Varolii to the point where the nerve escapes through the skull, can for a moment doubt that the motor root must have been cut through, in all these experiments. I repeat the question, therefore: Did M. Magendie draw attention to the effect which must inevitably have been produced, by his dividing both roots of the nerve at once, on the muscles of the jaws? This is very easily answered. Not a single observation is to be found in his account of these experiments, about the muscles of the jaws! In no part of his different memoirs can we discover any hint that the fifth pair is in part a motor nerve. However much it was dwelt upon in this country, that the nerve in question controlled the motions of the muscles of the jaws, besides conferring sensation on all the surfaces and cavities of the head, not a word was introduced by M. Magendie as to its power of presiding over the muscles!

It has already been said that, in giving the account of the above experiments, M. Magendie omitted to take any note of the roots, of which the nerve is composed. But his negligence in this respect was

not confined to the fifth pair. It was evinced no less remarkably in his mode of treating of the spinal nerves. With regard to these last-mentioned nerves, it cannot be doubted, that a principal argument in favour of our supposing that the one root has a different function from the other, is derived from the distinction in their anatomical structure—from perceiving that the root which confers motion has no ganglion formed upon it, while, on the contrary, that which gives sensation has a ganglion. And additional interest is attached to the contrast, when we compare the different roots, and the functions they possess, with the cerebral nerves. Upon finding that, among the nerves of the brain, the only one which bestows sensation (the larger root of the fifth) has a ganglion, while all the motor nerves are unprovided with such a structure, we are confirmed in our opinion that the roots of the spinal nerves which have corresponding structures, enjoy corresponding functions. But it is a remarkable fact, that in none of his different papers on the uses of the spinal nerves, did M. Magendie ever allude, even in a passing remark, to the anatomical distinction between the roots of these nerves. Judging from his memoirs, we should not be able to tell whether he was aware of the fact, that the posterior root had a ganglion, while the anterior had no ganglion. Again, as to the resemblance between the spinal nerves and any of the cerebral nerves, that is a subject he also left entirely out of consideration. Whether the fifth pair was correctly termed, in this country, the "spinal nerve of the head," or not,

he never made it an object to inquire. Indeed, it is altogether irreconcilable with the views he espoused concerning the fifth pair, to imagine that he had any conception of its being like the spinal nerves. The fifth pair, it was his object to shew, was the nerve of all the different organs of the senses; was the nerve of smelling, seeing, hearing, tasting, and touch; and how could he, therefore, suppose that such a nerve resembled the spinal nerves?*

In fine, having all these facts before us, can any one venture to assert that it was M. Magendie's aim

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* It is not to be forgotten that Mr. Mayo, when treating, in his first memoir (August 1822), of the functions of the fifth pair, omitted to speak of its double roots: thereby leading us to suppose that he was unacquainted, like most anatomists at the time, with that peculiarity of its structure. But in his second memoir (July 1823) he rectified that important omission, by introducing a correct description of the roots. Now, in doing so, he remarked, "that it was *well known* to anatomists, that the fifth nerve consisted at its origin of two roots."—See p. 106.

It was a full year after this (July 1824), that M. Magendie occupied himself with cutting across the fifth pair within the cranium; and neglected to mention that it consisted of two roots. Did that look as if the fact, that it consisted of two roots, had been "well known to anatomists?"

Again: in another of his works, when treating of the resemblance between the fifth pair and spinal nerves, Mr. Mayo spoke of it as "the *well-known* anatomical analogy between the fifth pair and spinal nerves" (p. 19). This he did although, on a previous occasion, he had attempted to confound the analogy, by saying that the par vagum bore as great a resemblance to the spinal nerves as the fifth pair. Now, if the analogy in question had been *well known* to anatomists, how did it happen that M. Magendie, three years after it had been repeatedly dwelt upon in this country, as of the greatest importance, was either ignorant of it, or would not acknowledge its truth?

to establish that the functions of the nerves correspond with their origins? Whether we take the roots of the spinal nerves, or the portio dura, or the fifth pair, and examine his views, we shall be convinced that he had a very opposite intention, in all his investigations. The anterior root of the spinal nerves he considered a compound root, inasmuch as he supposed that it ministered simultaneously to motion and sensation; the posterior root he likewise considered to be a compound root of the same nature; the portio dura, although arising by a single root, he thought combined motion with sensation; and as to the fifth pair, which, by universal consent, is regarded, at the present day, as possessing both motion and sensation, in correspondence with its two roots, he represented it as a single nerve, capable of supplying the place of all the different nerves of the senses: but he never mentioned whether it had the power of bestowing motion or not.

CHAPTER VI.

SIR CHARLES BELL'S THEORY OF THE "ORIGINAL" AND "RESPIRATORY" SYSTEMS OF NERVES.

I HAVE now brought forward all that has appeared necessary, to shew the respective parts taken by Sir Charles Bell and Mr. John Shaw on the one hand, and by Mr. Mayo and M. Magendie on the other, in originally propounding and confirming the general principle of these discoveries; namely, that the distinct functions of the nerves bear a relation to the divisions of the brain and spinal marrow from which they arise. But my task might be considered imperfectly fulfilled, if I did not offer a short explanation of the classification of the nerves proposed by Sir Charles Bell, in order to shew its consistency with what has been proved to be his original views.

Heretofore, I have made it an object to avoid entering on this part of the subject, because I have been desirous of keeping the inquiry as simple as possible. The distinction between the nerves to which I have sought to confine attention, has been that wide and easily understood distinction — between those nerves which bestow motion, and those which give sensation.

It does not require much reflection to perceive, that to establish that the nerve of motion is distinct

from the nerve of sensation, is a very different thing from deciding as to the particular nature of the nerves included under these names. Thus among the nerves of sensation, it is unquestionable that many modifications exist in the kinds of sensation which they possess; and so it is with the nerves of motion. But experience has proved, that it is no easy task to determine accurately the nature of these modifications.

Let me offer an example—chiefly for the purpose of shewing the necessity, in an inquiry such as the present, of using perfectly well-defined terms, in place of those open to dispute.

In treating of the nerves of motion, it is not uncommon for physiologists to attempt to distinguish those of *volition* from those that are *involuntary*. But when we ask what kinds of actions are to be considered voluntary, in contrast with involuntary, we shall find there is no correct line of division to enable us to judge of the distinction.

Thus, Dr. Wilson Phillip, who deservedly enjoys the respect of the profession for his writings on the nervous system, frequently adverts to the supposed distinction between the voluntary and involuntary nerves. But the following sentence, from one of his most recent productions, will prove, that he attaches a meaning to the former of these words "voluntary," that few other physiologists, it is to be presumed, would attach to it. He applies it to the act of breathing—an act which certainly, to say the least, is carried on independently of the will, quite as much as under the control of the

will; and he even represents the stertorous breathing of a patient labouring under apoplexy, as voluntary.

"Respiration," he observes, in his Gulstonian Lectures, "ceases on the destruction of the medulla oblongata; not because this organ either affords, or conveys to the spinal marrow, any part of its power, but because, on its destruction, the power of sensation, and consequently of *volition*, ceases, *respiration being, under all circumstances, an act of volition*. If the apoplectic breathes, when he can be excited to no *other act of volition*, it is because the want of air in the lungs at length becomes a more powerful stimulant than any other we can apply." *

Here, then, it is obvious, the term volition is used in a sense very different from what we should be disposed to allow is the correct acceptation of it. At all events, the above sentence proves what great freedom may be taken in interpreting its meaning. Now, it has been from observing the vagueness in the signification of these terms that I have wished, in treating of the distinction between the nerves of sensation and motion, to avoid touching upon the supposed distinction between the voluntary and involuntary nerves; and have thought it preferable to speak of the nerves of motion, simply as nerves whose office it is to control the muscles, whether those muscles act voluntarily or involuntarily, or, as some muscles do, partly in one way and partly in the other.

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* London Medical Gazette, vol. xvi. p. 261.

To shew the disadvantage of following a different course, and confounding the phrase, nerve of *volition* with nerve of *motion*, I must advert to the opinions of another writer.

Some time ago it occurred to the Rev. Mr. Whewell to present a history of the recent discoveries in the nervous system, which he communicated to a medical periodical.* The particular circumstances which induced this gentleman to engage in such a task it is needless now to explain. However, the main object he had in view was to establish, that for the discovery of the distinction between the nerves of motion and of sensation, the public was indebted to Mr. Mayo (conjointly with M. Magendie); and that it was not until it had been previously pointed out by these physiologists, that Sir Charles Bell became acquainted with the distinction.

Before proceeding further, I cannot resist expressing how much it is to be lamented that a gentleman so distinguished in the scientific world as Mr. Whewell, should have allowed himself, from any cause whatever, to mingle in this discussion. Throughout the whole of his letter containing the statement referred to, there is nothing but a succession of errors. One specimen I cannot help giving, to mark the extent of his ignorance concerning the individuals engaged in prosecuting the inquiries. In no part of his communication does he ever allude to Mr. John Shaw; consequently, he never mentions the numerous papers

* London Medical Gazette, vol. xxi. p. 526.

which that gentleman had addressed to the public, before Mr. Mayo contemplated writing on the nerves. Again, he refers invariably to Mr. Mayo, as Sir Charles Bell's chief competitor; but without being aware, it is obvious, that this gentleman had originally been Sir Charles's own pupil.

But putting these considerations aside, let me shew how it has happened that, by confounding the term nerve of *motion* with nerve of *volition*, in reference to the portio dura and fifth pair, Mr. Whewell has been led to suppose that it was reserved for Mr. Mayo to establish the distinction between the nerves of motion and sensation, before Sir Charles Bell was aware of there being such a distinction. The reader has to be reminded, that although the latter was aware of the portio dura being a nerve of *motion*, yet he refused to acknowledge that it was a nerve of *volition*, or to give it that name:—

"I must observe," Mr. Whewell remarks, "in the first place, that I have here nothing to do with other parts of Sir Charles Bell's labours, besides that which bears upon the discovery of the distinction between nerves of sensation and of *volition* . . . Confining myself, then, to the discovery of the distinction of sensitive and *motive* nerves, I must remark, &c. . . . The distinction of sensation and *volition* had, indeed, so long been familiar to the minds of physiologists, that no originality of thought was needed to hit upon it . . . But notwithstanding this conjecture, in his (Sir Charles Bell's) memoirs of 1821 and 1822, the reference of his researches is not to this difference of sensation and *motion*, but to another view resembling his original one. His main point in these

memoirs is, that certain nerves are nerves of sensation and *volition*, and that certain others are 'respiratory' nerves. Thus ('Phil. Trans.' 1821, p. 417), 'the fifth nerve, the nerve of *mastication* and sensation;' in 1822, p. 310, 'this (the respiratory) system of nerves is superadded to that of mere feeling and *agency*.'"

Accordingly, in the above sentences, Mr. Whewell uses the terms nerves of "volition" and "motion" as if they were synonymous. He even introduces the terms, nerve of "mastication" and "agency," as if these also had the same meaning as nerves of "volition." But leaving out of the question the two latter phrases, is it not surprising that Mr. Whewell should have committed such errors in logic as are evinced in the above quotations? How can he pretend to say that *motion* is the same thing as *volition?* Or, how could he expect, when confounding these terms together, to arrive at any logical conclusion? Need I say that "volition" denotes a particular condition of the mind, and in physiology is used in contradistinction to "involuntary," another word having reference to the mind; while "motion," so far from involving any idea connected with the mind or will, has reference oftener to inanimate than to living bodies; and, in physiology, is chiefly applied to muscular contraction and its effects? In short, to prove the wide difference between the two words, a nerve is a nerve of motion, whether it be a voluntary or involuntary nerve: but so far is it from being the case that every motor nerve is a nerve of volition, that it will be difficult to fix upon any

nerve in the body, and demonstrate that it is purely voluntary.

Let me therefore shew how Mr. Whewell, by confounding these two distinct terms, has been led to ascribe the chief honour of the discoveries to Mr. Mayo, instead of to Sir Charles Bell.

It has been repeatedly stated, that in consequence of observing the effects produced on the countenance, by destruction of the portio dura, either from wounds or disease, Sir Charles Bell had concluded that this was a *motor* nerve; and one that controlled both the voluntary and involuntary actions of the face. Whence he, very properly, avoided calling the portio dura a nerve of volition. On the contrary, he applied to it a name, "respiratory;" which, at the same time that it shewed it was a nerve of motion, did not involve the question of its being voluntary or involuntary. But Mr. Mayo, probably from confining his observations to the effects produced on the face of the ass—the favourite subject of his experiments, and an animal having little variety of expression in its countenance—arrived at a different conclusion. He maintained that the portio dura was *exclusively* a voluntary nerve, a nerve which in no way regulated involuntary action.

Now, it was from adopting this egregious blunder of Mr. Mayo's, as to the functions of the portio dura; and, moreover, from believing, that only a nerve of volition could be a nerve of motion, that Mr. Whewell has come to the conclusion, that Mr. Mayo was the first to establish the distinction between the nerves

of motion and of sensation! The following are his words:—

"Mr. Mayo, in his 'Anatomical and Physiological Commentaries' for August 1822 (p. 112), proposed objections to Sir Charles Bell's attempt to establish the difference of 'regular' and 'respiratory' nerves; and employed the familiar distinction of sensation and *volition* as the true interpretation of the results of his own experiments. Thus he asserted, that the portio dura of the seventh pair is a nerve of *voluntary motion;* and certain branches of the fifth pair, nerves of sensation only."

Accordingly, it cannot be doubted that it has been in consideration of Mr. Mayo's having given an account of the functions of the portio dura, which is absolutely and unquestionably wrong, and applied a name to that nerve altogether fallacious, that Mr. Whewell has ascribed to this gentleman the principal degree of praise in connexion with these researches!*

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* I may here remark that it is not in reference to the terms, motion and volition alone, that Mr. Whewell has exhibited a surprisingly loose mode of interpreting the meaning of words. To illustrate this point, I will present another extract from his communication to the "Medical Gazette:"—

"Looking upon the case," he remarks, "as above stated, with the best judgment I can use, I find myself led to the conclusion that no injustice was committed by joining Mr. Mayo's name to that of Sir Charles Bell as I did; but that, as I have already stated, this is not to be understood to imply that they had equal shares in the discovery. If I should have occasion to reproduce this part of my history, I should wish to describe the discovery as 'having been made by Sir Charles Bell, Mr. Mayo, and M. Magendie; the two latter physiologists having cor-

I will now endeavour, as briefly as possible, to explain the principal features of the arrangement of the nerves, with which Sir Charles Bell has been long occupied; and to which, it would appear, Mr. Whewell alludes in the above extract.

This arrangement was intended to embrace—with the exception of the nerves appropriated to the organs of the senses, and the sympathetic system—all the nerves of the body. That is to say, it included the whole of the nerves of the spine, together with the greater number of the cerebral nerves.

The chief object for which the classification was proposed, was to endeavour to solve certain problems

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rected and completed the researches of the former;' to which might be added the above notice of its confirmation. And it is to be observed, that this statement is independent of all the credit which may be due to Sir Charles Bell for his share in the discoveries respecting the functions of the nerves,—*vital, nutritive, sympathetic, instinctive, automatic, respiratory,* or *by whatever names they may be described,* which have been a main object of his labours."

In regard to the last sentence in this extract, I would observe, that *one* of the six words printed in italics, Sir Charles Bell has sometimes used in his writings. I mean the word 'respiratory.' For the other five,—I have read his works with some degree of attention, and for several years have heard him deliver his opinions at lecture; but neither in his writings nor his lectures do I recollect his ever using these particular terms. The word "sympathetic" he, of course, was obliged to employ in treating of the sympathetic system of nerves. But this system has never, hitherto, been a principal subject of his inquiries. In regard, therefore, to the words "vital," "nutritive," "instinctive," "automatic," I say confidently, that he never made use of them in any of his writings: and when Mr. Whewell affirms that "it was the main object of Sir Charles Bell's labours" to explain the nature of these nerves, I am forced to observe that he is under a gross and most extraordinary misapprehension.

which the anatomy of the nerves presented, and which the establishment of the fundamental principle, — namely, that the nerves possess distinct endowments in correspondence with their roots—appeared to hold out the means of solving.

The first, and most general of these problems, was the apparent *complexity* in the origin and distribution of the various nerves.

No one who has examined a minute dissection of the body can have failed to observe the different mode in which the nerves are distributed, as compared with the blood-vessels. In the circulating system, the arteries pass off directly from the heart by one common vessel; and the several organs receive their respective branches from the nearest arterial trunks, without any apparent complexity. One kind of fluid is conveyed from the central organ—the heart—to those parts; and one set of tubes is all that is required for its transmission. But in the nervous system, a difference is perceived. Instead of the various organs of the body deriving their nerves directly and immediately from a common source, it may happen that one particular part will receive a number of different nerves, each of which comes from a distinct part of the central organs — the brain and spinal marrow. And so far is it from being the case in the nervous, as it is in the vascular system, that the different organs are supplied by nerves coming off from the nearest, or what appear the most convenient points, of the central parts referred to, — they, on the contrary, obtain their appropriate nerves, as frequently as otherwise, from the remotest

divisions of the brain or spinal marrow. It is, in truth, to this circumstance, namely, that some of the nerves pass to their proper organs directly and by the shortest route, whilst others again proceed from a distance, crossing the former in their course, and interweaving with them, that the intricacy in the distribution of the nerves is principally owing.

Accordingly it may be perceived, that out of the view just given a distinct question arises: What is the meaning of the different parts of the frame obtaining their nerves in this apparently irregular manner? Why should an organ have several nerves coming from *different* parts of the brain or spinal marrow? Now, to this it may be answered,—that if the principle be correct, that the functions of the particular nerves correspond with the tracts of the brain or spinal marrow from which they arise, it is probable that the organs to which these various nerves tend, will, when properly examined, be found to enjoy distinct properties proportioned to the number of nerves which are sent to them; and that this may be the explanation of the difficulty.

To illustrate this part of the subject further:—Suppose that any particular organ in the body—let it be the tongue—is supplied by three distinct nerves, each of which proceeds from a different tract of the brain. One of the nerves we find passing to the surface of the organ; the second passing to certain muscles; and the third also passing to particular muscles. We commence by experimenting on each of these nerves. By cutting through the first, we ascertain that it is pro-

vided for bestowing sensation: accordingly, we trace it back to its origin, and are led to conclude that the tract in the brain, to which it passes, is appropriated to sensation. The second nerve is next subjected to experiment; and we discover that it is a motor nerve: wherefore we trace it back to its root, and infer that the column from which it arises is intended for controlling the motions of the muscles. Lastly, we experiment upon the third nerve, and it is found, as in the preceding instance, to be a motor nerve: we therefore follow it back to its root, and conclude that the tract from which it proceeds is destined, like the other just mentioned, for supplying motor power.

But here a difficulty (which direct experiments on the nerves will not remove) meets us. Why should two distinct nerves of motion be distributed to the same organ; or why should there exist two motor tracts in the brain? It seems irreconcilable with the simplicity evinced in the design of the animal frame, that when one tract or nerve might have answered the same object in the economy, there should be two; and it is inconsistent, likewise, with the principle, that different endowments reside in the various subdivisions of the brain and spinal marrow, to suppose that the parts alluded to should have the same identical functions.

In what manner are we to extricate ourselves from the difficulty here pointed out? It is by adopting two different courses of inquiry. First, since the direct experiments on the nerves just alluded to, only exhibit that there are two for motion, it is necessary to study with peculiar care the various associated actions of

the organ to which the nerves belong, for the purpose of ascertaining whether the muscular movements which it performs may not admit of being distinguished from each other in such a way as to account for its having two motor nerves. Secondly, we have to direct our particular attention to the tracts or columns of the brain from which the nerves referred to arise, so as to observe the various nerves which proceed from the same continued columns to other parts of the body. By examining these nerves, and investigating the functions of the organs to which they are appropriated, we may have it in our power, by comparison, to assign a common use to all those nerves which arise from the one tract; and a different use to all those which arise from the second tract. Hence we may happily, by such a train of observation, arrive at a distinct knowledge of the proper functions, not only of the several nerves, but also of the columns which compose the substance of the brain, — the final object to which all these inquiries point.

It was by following the course of reasoning here indicated, that Sir Charles Bell was led to form his peculiar opinions regarding the complexity exhibited in the distribution of the nerves, and the uses of the distinct columns of the brain from which they come off; as well as to frame that arrangement of the nerves which formed the subject of his first communication to the Royal Society.*

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* "The nerves of the animal frame are complex, in proportion to the variety of functions which the parts have to maintain. No organ which

Returning, then, after this explanation, to the observation of the general distribution of the nerves throughout the body. Since the *sympathetic* system of nerves, by universal consent, is admitted to have anatomical characters perfectly distinct from those of the cerebral or spinal nerves, we may be allowed for the present to postpone the consideration of its functions. When this is done, and the nerves appropriated to the organs of the senses, and their appendages, are also set apart, the questions demanding solution are narrowed. There remain only the extensive series of the

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possesses only one property or endowment has more than one nerve, however exquisite the sense or action may be ; but if two nerves, coming from different sources, are directed to one part, this is the sign of a double function performed by it . . . If a part or organ have many distinct nerves, we may be certain that, instead of having a mere accumulation of nervous power, it possesses distinct powers, or enters into different combinations, in proportion to the number of its nerves . . . Thus, in reviewing the comparative anatomy of the mouth, we shall find, that in creatures which do not breathe, the mouth having only one function to perform, one nerve is sufficient. In certain animals, where the face and nostrils have no complexity of relation, these parts have only a single nerve. If the throat has no complexity of organization, it has no variety of nerves. But on the other hand, when the anatomist employs weeks to dissect and disentangle the nerves of the tongue, throat, and palate, in the human subject, he finds at length that he has exhibited the branches of five different trunks of nerves : and there is no clue to the labyrinth, until he considers the multiplied offices of the mouth in man : that it is a pneumatic as much as a manducatory organ,—that it is the organ of voice and of speech, as of taste and exquisite feeling. It would, indeed, be matter of surprise if the same nerve served for the action of gnawing and feeding in the lower animals of simple structure, and also for the governance of those complicated operations which serve to interpret the wants and sentiments of man."—*Phil. Trans.*, July 1821.

spinal nerves, and a certain number of the nerves of the brain, to be submitted to examination.

When the nerves thus left for especial inquiry are viewed in a general manner, the first circumstance which arrests attention is the following remarkable contrast between them: namely, that a certain number are distinguished by having *two* separate origins from distinct tracts of the brain and spinal marrow; while the remaining nerves arise by *single* roots, from a tract distinct from either of those which give origin to the former set.

Hence, the anatomical structure of these nerves points to a natural division. First, there is one class, composed of nerves provided with two distinct roots. Secondly, there is another class of nerves provided with single roots. Now, although the nerves composing these two sets cross, join, and interweave with each other, so that in some parts they present a network, giving the appearance of the greatest disorder, we may, perhaps, by examining them separately, restore the subject to comparative simplicity.

1st. The nerves embraced in the first class, or class of double nerves, are the spinal nerves and the fifth pair. How are we to obtain such an exact knowledge of the functions of these nerves, as will enable us to understand why an additional class of nerves — the single-rooted nerves — should be conjoined to them, or superimposed on them? The only course we can pursue, is to examine accurately the nature of the particular organs which these double nerves supply.

After all that has been already said about the uses

of the spinal nerves and fifth pair, it cannot be questioned that, in correspondence with their two distinct origins, they bestow both motion and sensation. But which are the precise parts of the body that receive these two essential properties, viz. motion and sensation, through this class of nerves?

The spinal nerves and fifth pair, so far as *sensation* alone is considered, supply *all* parts of the body, from the crown of the head to the toe, with that property. But the same thing cannot be predicated of them in their capacity of giving *motion*. This is a point of great importance for the future argument. To the whole body, I repeat, this class of nerves gives sensation; but in the *head*, there is a difference with regard to the power of motion. Instead of the nerve, which in the head represents the spinal nerves, namely, the fifth pair, supplying promiscuously all the muscles situated there with the power of motion, it is restricted to giving motion to a particular class — to those muscles alone which co-operate in the act of seizing the food. Accordingly, the spinal nerves and fifth pair are provided for bestowing sensation on the body generally; motor power only on the trunk, the extremities, and the organ of *mastication*.

Let me therefore ask, What peculiarity can be observed in the constitution and plan of the frame of man or the higher animals, that will account for such a distribution of the nerves of this class; and more especially for one of them, the fifth, having its function so remarkably curtailed, as it were, in regard to the extent of its motor power?

If we confine our views to man, as he stands alone in the animal kingdom, we may not be able to give any plausible reason for this. But when we take an extended survey of living beings, and observe what are the particular organs that are most universally found, in the lowest as well as the highest of the animal kingdom, some light may fall on this subject.

To every creature ranking in the organic world above the vegetables — that is, to all animals, vertebrate or invertebrate, whatever may be the framework of their bodies, belong *sensation*, of some kind or degree—the power of *locomotion*, of some kind or degree—and an instrument for obtaining and *grasping the food*, of some kind or other. The mode of progression, the prehensile instrument, and the organ for consuming the prey, may differ in various animals, so far as the mechanism is concerned. But the first grand point of distinction between the vegetable and animal kingdoms is, that in the latter, the individuals *move* from spot to spot in search of nourishment, instead of obtaining it by roots fixed in the ground; and this power of locomotion, combined with that of appropriating the food, cannot be provided without the accompaniment of sensation. Aristotle's definition of animals, τα στῶματα ὄντα, cannot be received as perfect. It is an essential characteristic of animals, that, besides having *mouths* for the reception of their food, they shall likewise possess organs of locomotion and of prehension, as well as of sensation, to enable them to go in quest of, and to feel or taste, and to seize the food.

Hence we may now understand the reasons which

induced Sir Charles Bell to give to this class of nerves the name "Original System." This name was applied, in order to indicate that the fifth pair and spinal nerves formed a part of the nervous system which, in the lowest of the scale of animated beings as in the highest, bestowed those particular properties, which (independently of the digestive and assimilating organs) were the most necessary for existence. That is, the spinal nerves and fifth pair he considered typical of the nervous cord which, in an invertebrate animal like the leech (the instance he selected), superintends locomotion, the power of using its mandibles, and sensation.*

2. We are next drawn to attend to the remaining nerves,—those which, although they arise only by single roots, are nevertheless distributed to parts already profusely supplied with nerves, from the former class, composed of double roots.

Now, if we take the portio dura, the glosso-pharyngeal, the par vagum, and the spinal accessory nerves,

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* "When the nerves of the face, mouth, throat, and neck of the human subject, are minutely displayed, it seems impracticable to reduce the numerous nerves which cross and entwine with each other to two distinct classes: yet nothing is more certain than that such may be done, and by an easy and natural method. The principle which is to guide us is obtained by ascertaining what parts of the organization of an animal are necessary to life and motion, and what organs are superadded, as the animal advances in the scale of existence, as necessary to higher and more complex enjoyments and actions. Where an animal is endowed with mere sensation and locomotion—where there is no central organ of circulation, and no organ of respiration but what is generally diffused over the frame, the nerves are extremely simple: they consist of two

and observe the general course which they pursue, from their origins to their destinations, two remarkable circumstances will strike us. It will be perceived, on the one hand, that they all spring from one confined part of the brain, ascertained, by the experiments of Le Gallois, to exercise a direct and manifest influence over the function of breathing; and on the other hand, that they diverge extensively to the nostrils, mouth, palate, throat, side of the neck, glottis, larynx, trachea, bronchial tubes, lungs—in short, to the chief parts of the combined organ of respiration.

This general view, then, of the anatomy of these nerves, it will be admitted, affords sufficient grounds for inducing us to examine the organ of respiration, in the same manner as the parts supplied by the spinal nerves and fifth pair have just been examined: that is, to view this organ, not as it exists in man alone, but as it is found in the animal kingdom generally.

Now it is a fact, which cannot be called in ques-

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cords running in the length of the body, with branches going off laterally to the several divisions of the frame . . . and the central line of connexion is sufficient to combine the actions of the muscles, and to give them the concatenation necessary to locomotion.

"There is the same uniform and symmetrical system of nerves in the human body, as in the leech or worm . . . The nerves of the spine and the fifth, or trigeminus of the system of Willis, constitute this original and symmetrical system . . . . From the nerve that comes off from the anterior ganglion of the leech, and which supplies its mouth, we may trace up through the gradations of animals a nerve of taste and manducation, until we arrive at the complete distribution of the fifth or trigeminus in man."—*Phil. Trans.*, July 1821.

tion, that, of all the organs in the animal frame, there is no one which presents such remarkable differences in regard to its mechanism, when viewed in the lowest and the highest of the animal kingdom, as the organ of respiration. In the two great classes of the animal kingdom, the vertebrate and invertebrate, the apparatus of breathing presents such essential and important varieties, in its design and form, that to suppose the same system of nerves could be accommodated to both, bears, even upon its face, the appearance of inconsistency.

What are the chief points of difference, then, between the mechanism of the organ of breathing, in the higher and the lower animals? To expose the impure blood to the influence of the atmospheric air, I need not say, is the common object of respiration, in whatever mode it is accomplished. But let me point out how the same end is attained in the animal kingdom by two kinds of apparatus, the most distinct from each other that can be conceived.

In the most perfect animals, the blood is transmitted from a central organ of circulation — the heart, to a central organ for containing air — the lungs. These lungs, again, communicate with the atmosphere by one air-passage alone, the trachea. Accordingly, we have here what comparative anatomists denominate a "concentrated" apparatus of respiration — lungs, and a single windpipe; and it is a mechanism to be found in the vertebrate class of animals alone.

In what respect does this apparatus differ from that which is found in animals lowest in the scale of

existence — the invertebrate animals? In this most particular circumstance: — These lowest animals, instead of having lungs, have no lungs; and instead of having one tube alone, the trachea, communicating with the external air, have an infinite number of tubes, which, under the name of tracheæ or spiracula, open from the interior of their bodies, in all directions, upon their outward surface. To this last form of the respiratory organ, comparative anatomists, with the view of contrasting it with the "concentrated" apparatus, apply the name "diffuse" organ of respiration.

Accordingly, the question may now be put: Is it likely, since a difference of such a remarkable kind exists between the apparatus of breathing by a "concentrated," and by a "diffuse" system of respiration, that the distribution of the nerves will be the same in both — that there will be no corresponding modification in the arrangement of the nervous system?

Let the reader reflect on the nature of the difference. In the system of respiration by air tubes, as in the lower animals, the whole body of the animal constitutes its respiratory organ. Hence there is no particular part of the apparatus which requires to be guarded by peculiar sensibilities, against dangers threatening obstruction to the breathing. Neither can we imagine that there are any concatenated actions analogous to what we have in the higher animals. The air, on the contrary, enters freely into all parts of the body, by orifices kept permanently open. Besides, instead of a heart being provided to concentrate the flow of blood for oxygenation, to any one part, the circu-

lation, like the respiration, is *diffuse*. In the system of breathing by lungs, on the other hand, it is necessary, in the first place, that these lungs be lodged in a thorax. Again, it is necessary that the air tube, forming the only communication between the air and the lungs, be defended by a mechanism of the most perfect contrivance, and by sensibilities of the most lively kind, so as to prevent the intrusion of substances, the most minute of which conveyed by the atmosphere, might be sufficient to endanger life. Moreover, it is imperatively required that all the individual parts of this organ, however scattered, or distantly removed from each other, should be tied together by sympathies that will enable them to operate consentaneously; and on some occasions, with varied degrees of force. Hence we perceive, that for the alternate expansion and contraction of the lungs (motions independent altogether of loco-motion and those required for obtaining the food) and for the complex movements of the trachea, larynx, throat, mouth, and nostrils, distinct sets of muscles must be provided. But these muscles, before they can act, must be animated by nerves. Accordingly, a series of nerves, proportioned to the number of muscles newly introduced, must likewise be provided to perform that office. Now, it is not inconsistent with the principle which has been inculcated, to suppose that, in order to associate these nerves, and bestow upon them their peculiar endowments, a new and appropriate tract, or division, should be inserted into the brain and spinal marrow; from

which the nerves will be found, by dissection, to proceed. Such, at least, as far as I can understand Sir Charles Bell's theory, was the train of observation and reasoning by which he was led to apply to the particular nerves in question the name "superadded," or "respiratory;" and to suggest that there was a distinct column, called the "respiratory tract," in the spinal marrow and medulla oblongata.

The view here given of the arrangement in question, I am fully conscious, is but an imperfect one. To comprehend it properly would require me to take into consideration numerous questions on which I have not even entered. The relation, especially, existing between the organ of breathing and the circulating system, is one that demands the most particular study. In the venous circulation, it is known, great irregularities arise from the act of respiration. For example, when the chest is contracted during expiration, the current of blood returning to the heart is retarded, and the veins of the head and neck are turgid; whereas, when the chest is expanded during inspiration, the flow of blood is accelerated, and the veins are comparatively empty. Now these inequalities, especially in the disturbed condition of respiration, could not fail to produce injurious effects on the delicate textures of the eye and brain, if there were not certain provisions to prevent this tendency. Accordingly it will be found, that the actions of the muscles are arranged in such a manner as to guard these important organs from that source of injury: and the

actions referred to are dependent on the "respiratory nerves." Surely such a coincidence affords some presumption in favour of the views proposed. Again, if we were to examine the provisions that are introduced in these parts, to connect the function of breathing with deglutition; so that the latter act may be performed safely, without deranging the actions either of the larynx or of the diaphragm: or, lastly, if we were to consider how the organ of respiration is made subservient to voice and articulate speech; and how the face, neck, and chest, and the parts within the thorax, are agitated by strong emotions, giving rise to natural expression; the theory would gain additional confirmation. But for the objects I have at present in view, it is unnecessary, even if I were capable of doing perfect justice to these subjects, to bring them forward more fully than I have done.

This much, however, I submit, may always be affirmed in favour of the theory in question, whatever unwillingness there may be to adopt it as wholly proved, that it brings conspicuously forward, as subjects requiring investigation, certain problems in the anatomy which, I maintain, must undergo examination, and be satisfactorily solved, before it can be pretended that we are duly prepared for understanding the functions of the brain or spinal marrow. These are difficulties, moreover, with which, up to the present day, no physiologist has wrestled, but Sir Charles Bell himself.

I do not refer again, in making this remark, to the intricacy of the distribution of the nerves—espe-

cially in the face, neck, and thorax. But I cannot avoid drawing the reader's attention once more to the particular distribution of the portio dura and fifth pair, the nerves which have already so much occupied us. That physiologists should allow that, in the animal body, design and the highest wisdom are manifested throughout every part; and that to attribute any thing to chance, is only a different way of shewing our distrust in that proposition; and that they should, at the same time, have omitted to inquire into the meaning of the peculiar course pursued by these two nerves, appears to me a circumstance much to be wondered at.

The portio dura is a muscular nerve. It is a muscular nerve, too, of such a nature as to combine voluntary with involuntary power. It therefore appears capable of giving every kind of motion with which the muscles are endowed. Hence, the muscles of any part of the head, we might suppose, could be controlled by its influence. Nevertheless, anatomy and experiment prove indisputably that this is not the case. Why does the portio dura not give any branches to the muscles of the jaws? Trace this nerve from its root to its destination, and not a single branch will be found going to these muscles. When the portio dura escapes from the stylo-mastoidean foramen, below the ear, it lies close upon the two pterygoidei muscles; then winding over the ramus of the jaw, it expands, in the form of a plexus (the pes anserinus) upon the masseter muscle; next, diverging from this point, its branches traverse not only the masseter, but the

temporalis muscle; here a net-work of surprising minuteness of texture, as fine as lace,* is formed by the small branches given off from these larger trunks; but to none of all these muscles of the jaws, near which it thus passes, or on which it lies, does the nerve afford even one trifling twig! The anatomist might spend days in dissecting the portio dura, and he would not succeed in tracing even a filament into the muscles of mastication; and however often he repeated the experiment of cutting across this nerve in the living animal to ascertain its functions, he would not find that it exercised the smallest influence over these muscles of the jaws. The portio dura is limited to supplying those muscles alone which belong to the features. Now, why should this be the case?

On the other hand, in regard to the fifth pair. By the concurring testimony of anatomists, it is now admitted that this is the only nerve of the brain which, in its structure and functions, resembles the spinal nerves. Even our school-books call it "the spinal nerve of the head." But resembling the spinal nerves, as the fifth pair does, in this acknowledged manner, it differs from them in one remarkable circumstance. Taking the sensitive root, we can trace it to all parts of the head indiscriminately; that is, we find this portion of the nerve supplying the orbit,

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* Were the reader to inspect a preparation of the nerves of the face in my possession, dissected by Mr. C. W. Bell, he would not perhaps consider the description here given overdrawn.

cavities of the nose, mouth, tongue, and integuments of the face, indifferently. But in regard to the motor root it is otherwise. Instead of this portion being of the same dimensions as the sensitive root—as we find is the case with the analogous roots of the spinal nerves—it is stinted in size, and not a quarter as large; and, what is principally interesting, the branches of this motor root, in place of going indiscriminately to the muscles, are dedicated exclusively to those which combine to move the lower jaw in mastication.

Accordingly, those muscles which receive motor power from the fifth pair, do not receive a single filament from the portio dura; and on the other hand, where the portio dura supplies motion—there not the motor, but the sensitive root of the fifth, is alone distributed.

Now when we perceive this remarkable allotment of particular motor nerves, derived from distinct parts of the brain, to distinct classes of muscles, can we believe that it is the mere effect of chance? And if it be not an accidental arrangement, does not the distribution in question challenge an explanation? It was with the hope of supplying the required explanation, that Sir Charles Bell was encouraged to propose the theory and classification, of which I have offered a brief outline; and, until a satisfactory solution of these problems is supplied, no theory of the uses of the several divisions of the brain, from which the nerves proceed, will be of any value whatever.

I ought now, perhaps, in drawing to a close this statement, to solicit the reader's indulgence for having occupied him so long with questions which are almost wholly of a personal nature—relating more to who made the discoveries under discussion, than to the discoveries themselves.

But, in begging this indulgence, I cannot refrain from throwing out the following remark for consideration. On occasions of such improvements as those we have been discussing being achieved, the profession generally has a duty to perform. Doubtless the individual members composing our profession are under the sacred obligation to exercise their talents, so as to advance the common science in which all are immediately interested to the utmost degree. The profession, I repeat, has a claim upon individuals for these exertions; and to conceal or withhold any improvements which may have been made, whether relating to the administration of the doses of a medicine, or bearing upon questions of highest interest, affecting the general principles on which practice is founded, is a thing universally reprobated. This feeling, amongst men educated liberally, is a just one. But, allowing all this, it is inconsistent with fair dealing for the benefit to be all on one side. For the advantages it receives, the profession is under a bounden engagement to mete out with justice the only reward it has in its power to confer, upon those

members who have zealously and successfully wrought in its behalf; and the responsibility of cherishing and defending the reputations of these members, does not expire when the individuals who have been thus engaged in its service are removed by death.

The best intentions of the profession, under certain circumstances, may, I am conscious, be diverted, or rendered lukewarm. If a man, after having made a discovery which he considers of some merit, were to exhibit an overweening desire to obtain praise for it, and seize every opportunity of thrusting his own sagacity before the public, being jealous of others engaging in the same pursuits, the profession would do right to trouble itself little with such a person. Was there, however, any thing approaching to this, in the conduct either of Sir Charles Bell or Mr. John Shaw, when they first promulgated these discoveries? What was the first step which the former took? It was, in an unpublished essay, to lay down a general statement of the new principle, which, if properly prosecuted, he conceived would lead to the important improvements which have, in fact resulted from it; and to narrate, besides, experiments of the highest interest—those on the roots of the spinal nerves—to confirm that principle. This essay, in its title-page, expressed the object for which it was designed, namely, *to solicit other physiologists to join with the author in following out the new mode of investigation;* the idea of which had only recently suggested itself to his mind. As to the persons to whom copies of the work were transmitted, I happen to have in my

possession the original list of names; and it includes those of every physician, surgeon, or teacher of anatomy, in London, Edinburgh, or Dublin, who stood in any degree of eminence at the time it was printed. Now, because it so chanced that none of these distinguished individuals appreciated the merits of this work, or considered it worth their while repeating the experiments related in it, so as to verify the remarkable results, was Sir Charles Bell's conduct in requesting their co-operation the less to be commended, or the less liberal?

Again, if we view the next part of this gentleman's course, the same disposition to follow out his particular investigations, in a perfectly open manner, so as to allow others, if they pleased, to join in the same inquiries, will be perceived. It is to be remembered, that at the time referred to Sir Charles Bell, along with Mr. John Shaw, delivered annually two courses of anatomical lectures in the school of Great Windmill Street. To those who were pupils, it is also well known, that on the occasions when the former lectured upon the nervous system, the older students, settled in town, were drawn together, so as to form crowded classes, in the expectation of hearing him propound his views as they arose on these new subjects. But if this evidence be of no value, it is on record that when, by the observation of the resemblance between the fifth cerebral nerve and the spinal nerves, a new impulse was given to these researches, Mr. John Shaw repeated, without any attempt at concealment, in the presence of the pupils

of the school, the original experiments on the roots of the spinal nerves, in conjunction with those made for the first time on the cerebral nerves. This happened many months—more than half a-year—before any memoir treating of these discoveries was actually *published*. Hence, any student sitting in the class-room was not prevented retiring to his residence, setting down on paper the account of all that he had seen or heard, and proclaiming the discoveries as his own!

Now with regard to the gentlemen who are represented as having anticipated Sir Charles Bell, and whose names have been permitted to eclipse that of Mr. John Shaw, how did it stand with them? One, we learn, at the time referred to, namely, previous to any memoir having been actually published, was employed by Sir Charles Bell as a pupil, to assist in making dissections and preparations of the nerves, for the purpose of illustrating the lectures just mentioned. Did this betray any solicitude about admitting him to a knowledge of these pursuits? In three different papers this gentleman's name was mentioned, in a way that might have been gratifying to him, both by Sir Charles Bell and Mr. John Shaw, before he himself published his first memoir. Did this indicate any reluctance to do him justice before the profession? With regard to the other competitor, was there any concealment practised towards him? Even in this gentleman's own writings it is recorded, that, before he engaged in these pursuits, Mr. John Shaw in

person had explained to him the principle of these discoveries, and had exhibited to him the most interesting of the experiments intended to corroborate them. Could liberality have been carried to a higher pitch?

But, notwithstanding all these special, as well as general invitations, for other physiologists to join in prosecuting the researches, I have proved that there is not a single fact of value, amongst those which it has been attempted to wrest from Sir Charles Bell, that will not be found clearly enunciated, either in this gentleman's own writings or those of Mr. John Shaw, antecedently to any other individuals, at home or abroad, publishing on the Nervous System.

APPENDIX.

APPENDIX.

THE RETINA POSSESSES AN APPROPRIATE SENSE; DISTINCT FROM THAT OF A NERVE OF TOUCH.

I HAVE reserved, for a separate note, another example of the success that seems invariably to have waited on M. Magendie, when he has happened to adopt the observations of others, and promulgate them as his own.

It has been seen, at p. 35, that, so far back as 1811, Sir Charles Bell had pointed out, in his "Idea of a New Anatomy of the Brain," the interesting fact, that the nerves of the senses are incapable of receiving any other impressions but such as are adapted to their particular organs. Thus he shewed, that in whatever manner the nerve of vision was mechanically injured, the only sensation it could give rise to had relation to the varieties of light and colour. And his proposition he illustrated by a simple yet convincing experiment. He took the example of what occurs in couching a patient for cataract. When the needle, in this operation, pierces the conjunctiva, which is endowed with acute sensibility, for the protection of the eyeball, the patient experiences pain; that is, a sensation similar to what belongs to the surface of the body generally.

But when the needle enters deeper, and transfixes the retina, the kind of sensation is altogether different: a flash of light is produced. From these facts he argued that the retina was insensible; at least, according to the common acceptation of that word.

"When, in contemplating the structure of the eye, we say, how admirably it is adapted to the laws of light, we use language which implies a partial, and, consequently, an erroneous view. And the philosopher takes not a more enlarged survey of nature, when he declares how curiously the laws of light are adapted to the constitution of the eye. This creation, of which we are a part, has not been formed in parts. The organ of vision, and the matter or influence carried to the organ, and the qualities of bodies with which we are acquainted through it, are parts of a system great beyond our imperfect comprehension; formed, as it would seem, at once in wisdom, not pieced together like the work of human ingenuity. When this whole was created (of which the remote planetary system, as well as our bodies, and the objects more familiar to our observation, are but parts), the mind was placed in a body not merely suited to its wisdom, but in circumstances to be moved by the materials around it: and the capacities of the mind, and the powers of the organs, which are as a medium betwixt the mind and the external world, have an original constitution framed in relation to the qualities of things.

"In this inquiry it is most essential to observe, that while each organ of sense is provided with a capacity of receiving certain changes to be played upon it, as it were, yet each is utterly incapable of receiving the impressions destined for another organ of sensation. It is also very

remarkable, that an impression made upon two different nerves of sense, though with the same instrument, will produce two distinct sensations: and the ideas resulting will only have relation to the organ affected. * * *

"In the operation of couching the cataract, the pain of piercing the retina with a needle is not so great as that which proceeds from a grain of sand under the eyelid. And although the derangement of the stomach sometimes marks the injury of an organ so delicate, yet the pain is occasioned by piercing the outward coat, not by the affection of the expanded nerve of vision. If the sensation of light were conveyed to us by the retina, the organ of vision, in consequence of that organ being as much more sensible than the surface of the body as the impression of light is more delicate than that pressure which gives us the sense of touch, what would be the feelings of a man subjected to an operation in which a needle were pushed through the nerve?—Life could not bear so great a pain.

"But there is an occurrence during this operation on the eye, which will direct us to the truth: when the needle pierces the eye, the patient has the sensation of a spark of fire before the eye. This fact is corroborated by experiments made on the eye. When the eyeball is pressed on the side, we perceive various-coloured light. Indeed, the mere effect of a blow upon the head might inform us that sensation depends on the exercise of the organ affected, not on the impression conveyed to the external organ; for, by the vibration caused by the blow, the ears ring, and the eye flashes light, while there is neither light nor sound present."—(P. 12.)

These views were afterwards reproduced by Sir Charles Bell, in a paper read before the Royal Society, June 1823; and no one can doubt that it was

his object to prove that the sensibility belonging to the retina was of a totally different kind from that of common *touch*. I subjoin the passage to which I allude:—

"This notion of a fluid moving backwards and forwards in the tubes of the nerves, equally adapted to produce motion and sensation, has perpetuated the error that the different nerves of sensation are appropriated to their offices by the textures of their extremities; 'that there exists a certain relation between the softness of the nervous extremities and the nature of the bodies which produce an impression on them.' On the contrary, every nerve of sense is limited in its exercise, and can minister to certain perceptions only. Whatever may be the nature of the impulse communicated to a nerve—pressure, vibration, heat, electricity—the perception excited in the mind will have reference to the organ exercised, not to the impression made upon it. Fire will not give the sensation of heat to any nerve but that appropriated to the surface. However delicate the retina be, it does not feel like the skin. The point which pricks the skin being thrust against the retina, will cause a spark of fire or a flash of light. The tongue enjoys two senses—touch and taste: but by selecting the extremity of a particular nerve, or, what is the same thing, a particular papilla, we can exercise either the one or the other sense separately. If we press a needle against a nerve of touch, we shall feel the sharpness, and know the part of the tongue in contact with the point: but if we touch a nerve of taste, we shall have no perception of form or of place, we shall experience a metallic taste."

Now, the very same observations as those contained in the above quotations, were afterwards brought for-

ward by M. Magendie, as facts newly discovered by himself. The following extracts are from this gentleman's "Journal of Physiology," dated May 1824; that is, eleven months after the publication of the paper from which the last of the quotations was taken. It has also to be observed that, some time before this, M. Magendie had in his possession a copy of the "Idea of a New Anatomy of the Brain," from which the former quotation was taken; for he acknowledged, in his "Journal" of October 1822, having received this work from Mr. John Shaw, on the occasion of his claiming originality for the experiments on the roots of the spinal nerves:—

"In a previous number (May 1824) I expressed my doubts as to the retina being endowed with sensibility: but, from the manner in which I performed my experiments then, I had not seen the phenomena in their true light. I had punctured the eye on its forepart, the surface of which, as every one knows, is acutely sensible; but, by doing so, the needle, of course, continued to be in contact with the conjunctiva; and as I moved the instrument within the eye, this gave rise to some degree of pain. These signs of pain led me to conclude that the retina possessed a slight degree of sensibility. I have since that time avoided those sources of error, by puncturing the eye from behind; and I now feel assured that, however much the retina be torn or pricked, the animal will give no indications of suffering. Thus we arrive at a conclusion that may be called a paradox: the retina is insensible, and, strictly speaking, in the rabbit, is not endowed with general sensibility, but with a sensibility of a special kind related to vision."

M. Magendie then details the results of his experiments on the human subject! And who will not be astonished at the unwarrantable freedom used by him towards his patients?

"About four months ago, a woman, of middle age, was brought to the Central Office of Hospitals, affected with cataract in both her eyes, and anxious to submit to an operation. I thought this a favourable opportunity to satisfy myself whether it were really true that the retina possessed that high degree of sensibility which both physiologists and metaphysicians have usually ascribed to it. I had often seen the operation of couching, and had frequently performed it myself, without observing that pain was evinced when the lens was depressed and brought in contact with the retina. Yet, if the retina possessed the exquisite sensibility which it is said to enjoy, there would undoubtedly have been pain at that time.

"After due preparation I proceeded, accordingly, to depress the lens in the usual manner. The cataract was soft; and I found it necessary to depress it piece by piece. Each time that I carried a portion downwards, I felt distinctly the resistance of the walls of the eye-ball, and was satisfied, therefore, that it pressed against the retina. Nevertheless, the patient uttered no complaint, and did not seem aware of what had been done. Emboldened by this, *I directed the needle towards the central part of the bottom of the eye, and lightly touched the retina with the point.* The woman gave no indications of pain. *I repeated this experiment five or six times,* always with the same result; and it was obvious that the retina was not paralysed: for on each occasion when I touched it, the woman expressed her *raptures at again seeing the light.*

"The same series of observations was repeated on the other eye; in which the cataract was also soft. I proceeded, however, with more boldness. Having first depressed the lens as completely as I could, *I then touched the retina freely (franchement) in several distinct places, with the point of the instrument;* and in doing this, I am sure, pushed it sufficiently far *to perforate the membrane at each of the different parts:* but still, the patient was unconscious of what I was doing.

"I have lately had the opportunity of repeating the same observations on a patient affected with cataract in the right eye, at the Hôpital de la Pitié. The lens was easily depressed; and the sight was immediately restored. *I then touched the retina with the needle in several distinct places,* but without the patient experiencing any pain. The contact of the instrument with the retina, and even the pricks that I made, did not interfere with the success of the operation.*

As soon as these experiments were announced, they went the round of the scientific journals of the day, as ingenious experiments, originated by M. Magendie. In his "Elements of Physiology," now in progress of publication, they are thus noticed by M. Müller:—

"It is a question," he says, "whether the sensation of pain may not be felt in the nerves of the higher senses? whether, for example, violent irritation of the optic nerve may not give rise to the sensation of pain? The question is difficult of solution. To determine the question, it is neces-

* Journ. de Physiol. Exp., tom. v. p. 38. 1825.

sary to institute experiments on the isolated nerves of speeial sense themselves . . . . The olfaetory nerves laid bare in the dog evinee, when prieked, as Magendie observed, no sign of common sensibility; and the retina and optie nerve were also, in Magendie's experiments, insusceptible of pain from meehanieal injury."*

Accordingly, without adverting to Sir Charles Bell, M. Müller ascribes the originality of these observations to M. Magendie exclusively. But putting the question of priority altogether aside, I must be allowed — with all deference to M. Müller—to object to any importance being attached to the experiments on the *olfactory* nerves alluded to above. It is not possible, in my opinion, to conceive any thing more discreditable to physiology than these experiments.

In the first place, M. Magendie endeavoured to establish, by means of them, that the power of distinguishing odours could be retained by the branches of the fifth pair, after the olfactory nerves had been destroyed; in other words, that the sense of smelling belonged to the fifth pair, instead of to the olfactory nerve. Now, to any one who will take the pains to examine his experiments carefully, it will be obvious that the particular stimuli which he applied to the animals, the subjects of his experiments, in order to ascertain this point, only produced an effect by acting on the naturally sensitive membrane of the nose; or by exciting a sensibility which this membrane owes to

* Müller's Physiology, translated by Baly, p. 1069. See also pp. 767, 770.

the branches of the fifth pair, in common with other parts of the head supplied by that nerve, and which M. Magendie mistook for smelling.

Secondly, when we consider the delicate nature of the questions to be resolved, who would place any reliance on experiments of such a coarse and cruel description as those M. Magendie performed? For example, the first step of his operations for laying bare the olfactory nerves, was to turn out the brain from its natural cavity!

Thirdly, the animals which he selected for his experiments were incapable, even if they had not been so much mutilated, of signifying, in any intelligible way, what was the particular kind or degree of sensation which they experienced when under his hands. They were, according to his own minute account of them, "chickens, ducks, magpies, puppies, kittens, guinea-pigs." *

Lastly, as if to clothe his whole proceedings with absurdity, after exhausting the supposed proofs derived from experimenting on the chickens, ducks, &c., he had recourse to the evidence of persons as incapable of giving a satisfactory account of the sensations they experienced, as the animals referred to would have been, had they been endowed with language. He adduced the testimony of a M. Ramon, the keeper of a lunatic asylum, to prove that patients afflicted with *insanity* retained the sense of smelling in its integrity,

* Journal de Physiologie, tom. iv. p. 174.

although not only the olfactory nerves, but the anterior lobes of the brain, were extensively disorganized by disease!

When will such experiments, at once so unscientific, so useless, and so offensive to humane feelings, have an end?

THE END.

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